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THE WARMING-UP EFFECT

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THE PROBLEM

The warming-up effect is usually mentioned in discussions of the work curve, but little or nothing is said about the conditions which determine its appearance. Thorndike[1] seems to feel that the importance of this characteristic of the work curve has been exaggerated, and that the evidence for it is mostly indirect. He points out that in most of the data bearing upon this subject it is almost impossible to separate warming-up (by definition a very temporary effect) from "the more permanent improvement that comes from exercise of the function in general." Watson[2] states that experimental confirmation of the warming-up effect is not at hand either for vocal or manual functions. He does say, however, that "in baseball, track and crew work a process of limbering up or preliminary practice is universally indulged in and is apparently necessary."

We believe that something more definite can be said about the effect under consideration, at least in so far as it is a feature of the *speed* or *amount* curve. Warming-up is a rise in efficiency which is steeper and more temporary than the rise which can be seen in, let us say, successive daily performances. Now it has frequently been pointed out that

[1] 'Educational Psychology,' Vol. III., 66–68.
[2] Psychology, 354–355.

there is another characteristic of the work curve, *i.e.*, the general decrement, or fatigue effect, which, if it is present, tends very naturally to obscure any such opposite effect as warming-up. Since these two effects, warming-up and the general decrement, are opposite, it should be possible to find controllable conditions where the factors producing either one are so favored that that effect obscures or diminishes the other. This, we believe, can be done. We believe it can be demonstrated that warming-up may fail to appear as a feature of the curve for a given kind of work simply because that work is too continuous, and that this effect may be made apparent by introducing a proper degree of discontinuity into the work in question.

The Wells Tapping Experiment

The first experiment that we shall cite is a study by Wells of rate of tapping.[1] Five 30-second periods of tapping were separated from each other by intervals of 2½ minutes. Considering the total number of taps made by the right hand in the successive 30-second periods, there is a consistent improvement or warming-up effect. But if we consider the 6 successive 5-second periods within each 30-second period of continuous tapping, we find a loss rather than a gain in efficiency. In other words, the curve of work possessing a certain discontinuity (in this case where there is a 2½-minute rest every 30 seconds) may show a definite warming-up effect, while the curve of more continuous work (in this case 30 seconds of continuous tapping) shows a general decrement.

In the case of Wells' experiment, it is not difficult to separate this warming-up effect, the appearance of which is dependent upon a certain degree of discontinuity in the work, from "the more permanent improvement that comes from the exercise of the function in general."

1. In a group of ten subjects who worked at this experiment on two days, the amount of improvement for the right hand within a day's work was greater than that of the second

[1] 'Normal Performance in the Tapping Test, etc.,' *Amer. Journ. of Psychol.* 1908, 19, 437–483.

day over the first, even though the latter was favored by more practice. (See Table I, pp. 441–442.) The averages for successive 30-second periods on the first day were 192.4, 190.5, 193.7, 198.8, 199.2, whereas the average for all of the first day was 194.9 for 30 seconds, and for the second day 193.7, indicating a loss rather than a gain from day to day. The temporary improvement called warming-up is certainly of a more marked nature here than any positive effects of general practice.

2. Two subjects worked at the experiment for thirty days. The warming-up effect became more apparent during the later days of the experiment when day to day improvement had practically ceased, or certainly had not manifested the positive acceleration which would be required if the warming-up effect and the effect of general practice were to be identified. (Plate II, p. 460.)

3. The ten subjects mentioned in (1) also tapped with the left hand. While their improvement from one day to the next may be represented by the values 173.0–176.3 (Table I, pp. 442–443), there is no warming-up effect within either day.

All of these facts indicate the distinction between the more general day to day improvement and the more temporary warming-up. Evidently either may be present without the other or to a notably greater degree than the other.

Other Evidence from the Literature

Kraepelin and his students[1] have presented a number of experiments where work interrupted by shorter rest periods was more efficient than that interrupted by longer rest periods. The argument is that rest does two things: it eliminates fatigue and it destroys the advantageous warming-up effect. There is in some cases a particular length of rest which, while it is great enough largely to eliminate fatigue, is not so great that it eliminates warming-up. Therefore it is to be expected that the optimal length of rest will lie between a greater length and a lesser length. It is not necessary to assume that rest always destroys the fatigue effect at a

[1] Arai, 'Mental Fatigue,' 16–28, reviews in some detail the studies of work made by Kraepelin and his students. She also gives a bibliography.

greater rate than it destroys the warming-up effect, but merely that it does so in certain cases. This argument points in the direction of our main contention, *i.e.*, that a certain degree of discontinuity makes the appearance of the warming-up effect more probable. The same is true of the work of Wimms[1] who found that a 10-minute rest between two 10-minute periods of addition or multiplication was better than a 0-minute rest or a 20-minute rest.

Batty's[2] subjects tapped for five successive 5-second periods with rests of 5, 10, or 20 seconds between periods. There was a loss from period to period except with 20-second rests, under which condition the right hand showed improvement. Part of this improvement may be warming-up effect saved by the discontinuity of the work, but in this experiment there is, unfortunately, no way of separating the saving of warming-up effect from that of practice.

Two Experiments on Tapping

Experiment I

In the first of our experiments, 12 subjects, students in an advanced course in experimental psychology, performed at the tapping test. The ordinary tapping board and clock counter furnished by Stoelting were employed. This apparatus is not reliable for measuring the absolute number of taps, but with subjects who have had enough preliminary training to establish a tapping movement of good amplitude, the apparatus can be used satisfactorily for obtaining relative values. Our subjects had such preliminary training, and we were interested only in relative values.

The experiment contained the six conditions that follow:

Condition I—Tap 5 minutes, rest 0 seconds, tap 5 minutes
Condition II—Tap 5 minutes, rest 10 seconds, tap 5 minutes
Condition III—Tap 5 minutes, rest 20 seconds, tap 5 minutes
Condition IV—Tap 5 minutes, rest 40 seconds, tap 5 minutes

[1] 'The Relative Effects of Fatigue and Practice Produced by Different Kinds of Mental Work,' *Brit. Journ. of Psychol.*, 2, 1906–1908, 153–195.

[2] 'Some Observations Upon Practice and Fatigue as They Affect the Rate of Tapping,' *Amer. Journ. of Psychol.*, 20, 1909, 449–455.

Condition V—Tap 5 minutes, rest 80 seconds, tap 5 minutes
Condition VI—Tap 5 minutes, rest 120 seconds, tap 5 minutes

There were two cycles in the experiment, *i.e.*, each subject went through every condition twice. The different subjects, because of the usual considerations of practice and the like, went through the conditions in varying order. A subject never worked under more than one condition on any one day.

If there is a warming-up effect demonstrable by this experiment, more work should be done under certain of the above conditions in the second five minutes of tapping than in the first five minutes. Furthermore, this improvement should be greater than that which takes place from day to day.

In order to discover whether this expectation is realized, it is necessary to know:

1. The total number of taps recorded for the 12 subjects during the first 5 minutes for each experimental condition. This value we shall call X.

2. The total number of taps recorded for the 12 subjects during the second 5-minute tapping period for each experimental condition. This value we shall call Y.

3. The total number of taps recorded for the 12 subjects during the first 5 minutes of tapping for the experiment day immediately following that on which occurred the experimental condition under consideration.[1] This value we shall call Z.

The X, Y, and Z values for the six conditions and the two cycles are shown in Table I.

Now the amount of warming-up, uncorrected for effects of practice, is the excess of the number of taps made in the second five minutes of tapping in a given experimental condition over the number made in the first five minutes of the same condition. This can be expressed conveniently in percentage terms as

$$\frac{100(y - x).}{x}$$

[1] In order to get this value in the second cycle for every experimental condition, it was necessary to take one 5-minute record beyond those in the regular plan of the experiment.

TABLE I

VALUES OF X, Y, AND Z FOR 12 SUBJECTS IN EACH CONDITION FOR TWO CYCLES

	X	Y	Z
Cycle 1			
Condition I	23213	22817	23673
" II	23274	22896	23395
" III	23085	23160	23894
" IV	23341	23783	23521
" V	23289	23960	23314
" VI	22852	23356	23213
Cycle 2			
Condition I	23284	24207	24596
" II	24473	24325	24367
" III	24134	24479	25118
" IV	25191	25423	24952
" V	24699	26009	25034
" VI	24812	25552	24530

The amount of practice, as distinguished from warming-up, is the excess of the taps made during a first 5 minutes on the experiment day succeeding that on which the condition in question occurred over the taps made during the first 5 minutes of the condition in question. This can be expressed in percentage terms as

$$\frac{100(z - x).}{x}$$

The warming-up may be said to be demonstrated where the increase from a first to a second five minutes of tapping is greater than that from a first five minutes of tapping on one experiment day to the first five minutes on the immediately succeeding experiment day. In other words, the warming-up effect is present to the degree that

$$\frac{100(y - x)}{x} - \frac{100(z - x)}{x} > 0,$$

or

$$\frac{100(y - x)}{x} > \frac{100(z - x).}{x}$$

Our test of the presence and degree of the warming-up effect, if it is in error, errs on the side of too great severity. The amount of practice effect is measured by improvement from the first part of one experiment day to the first part of

the next. The practice which may be assumed to affect that next day's tapping is that included not only in the first 5 minutes of one day, but also in the second 5 minutes of that day. The practice which affects the second five minute period of an experiment day as compared with the first is, however, only that included within a single five minutes of tapping.

The values $\frac{100(y-x)}{x} - \frac{100(z-x)}{x}$ for the six conditions and the two cycles are shown in Table II.

TABLE II

SHOWING DIFFERENCES BETWEEN WARMING-UP AND PRACTICE: $\frac{100(y-x)}{x} - \frac{100(z-x)}{x}$

	Condition					
	I	II	III	IV	V	VI
Cycle 1.....	−3.6	−2.1	−3.2	1.1	2.8	.6
Cycle 2.....	−1.7	− .2	−2.7	1.8	3.9	4.0
Av. (2 cycles).....	−2.6	−1.1	−2.9	1.4	3.3	2.3

The two cycles of the experiment agree in showing an absence of the warming-up effect for the conditions with 0, 10, and 20 seconds rest, and the presence of that effect for the conditions with 40, 80, and 120 seconds rest. The relative continuity of work under the first three conditions evidently so favors some such negative tendency as the fatigue effect that the factors which operate to produce the warming-up effect fail to do so. The relative discontinuity of the other three conditions saves the warming-up effect.

Experiment II

In our second experiment we again employed 12 advanced students. None of them was familiar with the first experiment. They all had several practice trials at tapping before the experiment began.

The experimental conditions were as follows:

Condition I.—Tap 5 minutes, rest 0 seconds, tap 5 minutes

Condition II.—Tap 5 minutes, rest 80 seconds, tap 5 minutes
Condition III.—Tap 5 minutes, rest 1200 seconds, tap 5 minutes

Each subject went through these conditions four times, and in addition had a five minute tapping period from which we could calculate the magnitude of the practice effect at the time of the last regular experimental test. Again, the different subjects went through the conditions in varying order and, again, only one test was made on any one experiment day.

The data were handled in the same way as those of Experiment I. The symbols X, Y, and Z also have the same meaning.

The values of X, Y, and Z appear in Table III.

TABLE III

VALUES OF X, Y, AND Z FOR 12 SUBJECTS IN EACH CONDITION FOR FOUR CYCLES

	X	Y	Z
Cycle 1			
Condition I	23982	23072	24095
" II	24232	24388	24228
" III	24091	24555	24043
Cycle 2			
Condition I	24616	24341	24564
" II	24236	25048	24613
" III	24427	25392	24677
Cycle 3			
Condition I	24702	24559	25226
" II	24929	25776	25188
" III	24925	25772	25093
Cycle 4			
Condition I	25520	25066	25530
" II	25549	25780	25226
" III	25366	26208	25458

Table IV contains the values of $\frac{100(y-x)}{x} - \frac{100(z-x)}{x}$.

All cycles of this experiment, like those of the first, agree in showing an absence of the warming-up effect where the work is most continuous (with 0 seconds rest) and the presence of that effect where the work has a certain discontinuity.

These two experiments on rate of tapping confirm in a new setting the fact, apparent in Wells' data, that whether

or not the warming-up effect is present in tapping depends upon the relative continuity of the work. If the work is too continuous, as where it is uninterrupted, the fatigue effect is maximally favored and the warming-up effect is concealed, even if, as is very likely the case, the factors which tend to produce it are present in the work. If, on the other hand, the work is too discontinuous, as where the earlier and later portion of it are separated by a day or more, the warming-up effect is also lost. For such a function as tapping, there seems to be a medium degree of continuity or discontinuity which maximally favors the warming-up effect.

TABLE IV

SHOWING DIFFERENCES BETWEEN WARMING-UP AND PRACTICE:

$$\frac{100(y-x)}{x} - \frac{100(z-x)}{x}$$

	Condition		
	I	II	III
Cycle 1	−4.3	.7	2.1
Cycle 2	− .9	1.8	2.9
Cycle 3	−2.7	2.4	2.7
Cycle 4	−1.8	2.2	2.9
Average (4 cycles)	−2.4	1.8	2.7

THE WARMING-UP EFFECT IN VOCAL WORK

Experiment III

The activity studied in this experiment was the recitation of the alphabet backward. Five advanced students in experimental psychology served as subjects.

On three preliminary days the subjects were given twenty trials a day at reciting the alphabet backward. Rests were given between the trials. Following this, on each of four days each subject recited the alphabet backward for twenty minutes without rest. Both speed and accuracy were recorded, but the errors were so few that we decided to neglect them during the main experiment.

The main experiment began as soon as the program described in the last paragraph was completed. The conditions were:

Condition I—Recite the alphabet backward continuously for a period of 20 minutes

Condition II—Recite for 30 seconds, rest for 15 seconds, recite 30 seconds, rest 15 seconds, etc. until the time of actual recitation totals 20 minutes

Condition III—Recite for 30 seconds, rest for 30 seconds, recite 30 seconds, rest 30 seconds, etc. until the time of actual recitation totals 20 minutes

Each subject went through each condition four times. Only one test was made on any single day.

The records of the subject were scored in terms of letters recited during each of the 40 half-minute periods making up the total 20 minutes of recitation. The experimenter made a mark for every recitation of the letter A and wrote down the actual letter which the subject had reached at the end of a half-minute period. From this record it was easy to calculate the number of letters recited in each successive half-minute of the work.

Figs. 1 to 5 show the work curves for the different subjects under the different conditions of the experiment. These curves were constructed from the sums of the amounts of work done in each successive 30-second work period during the four repetitions of the experiment.

A sharp rise or warming-up effect, more marked than subsequent irregularities, is shown in the early parts of all of the curves for subject 1, in all of the curves for subject 2, in the Condition III. curve for subject 3, in the Condition II. and III. curves for subject 4, and in the Condition II. and III. curves for subject 5. Certain of the other curves show a rise in their early portions, but these rises are no more marked than others appearing in later portions of these curves, and so should hardly be given special treatment.

Mere inspection indicates that those cases of initial rise which we have accepted as instances of warming-up are not due to practice in the ordinary sense of the word. They are much too sharp for that. We have, moreover, some figures which will indicate the contrast between the warming-up effect and practice in this function. In every case where

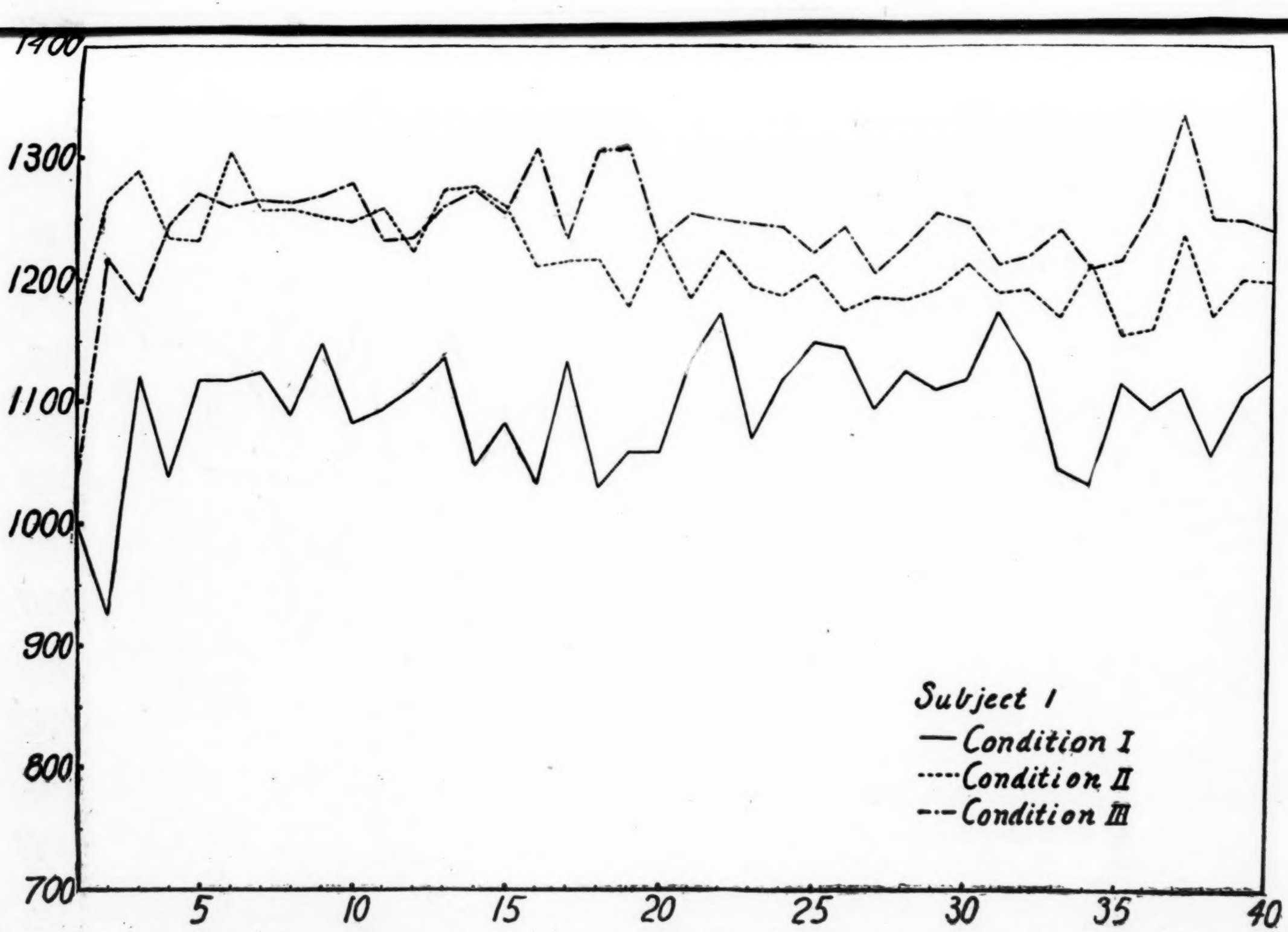

FIG. 1. Horizontal axis represents the successive 30-second periods making up the total 20 minutes of recitation. Vertical axis represents the sums of the letters recited during each of the 30-second periods for 4 complete recitations.

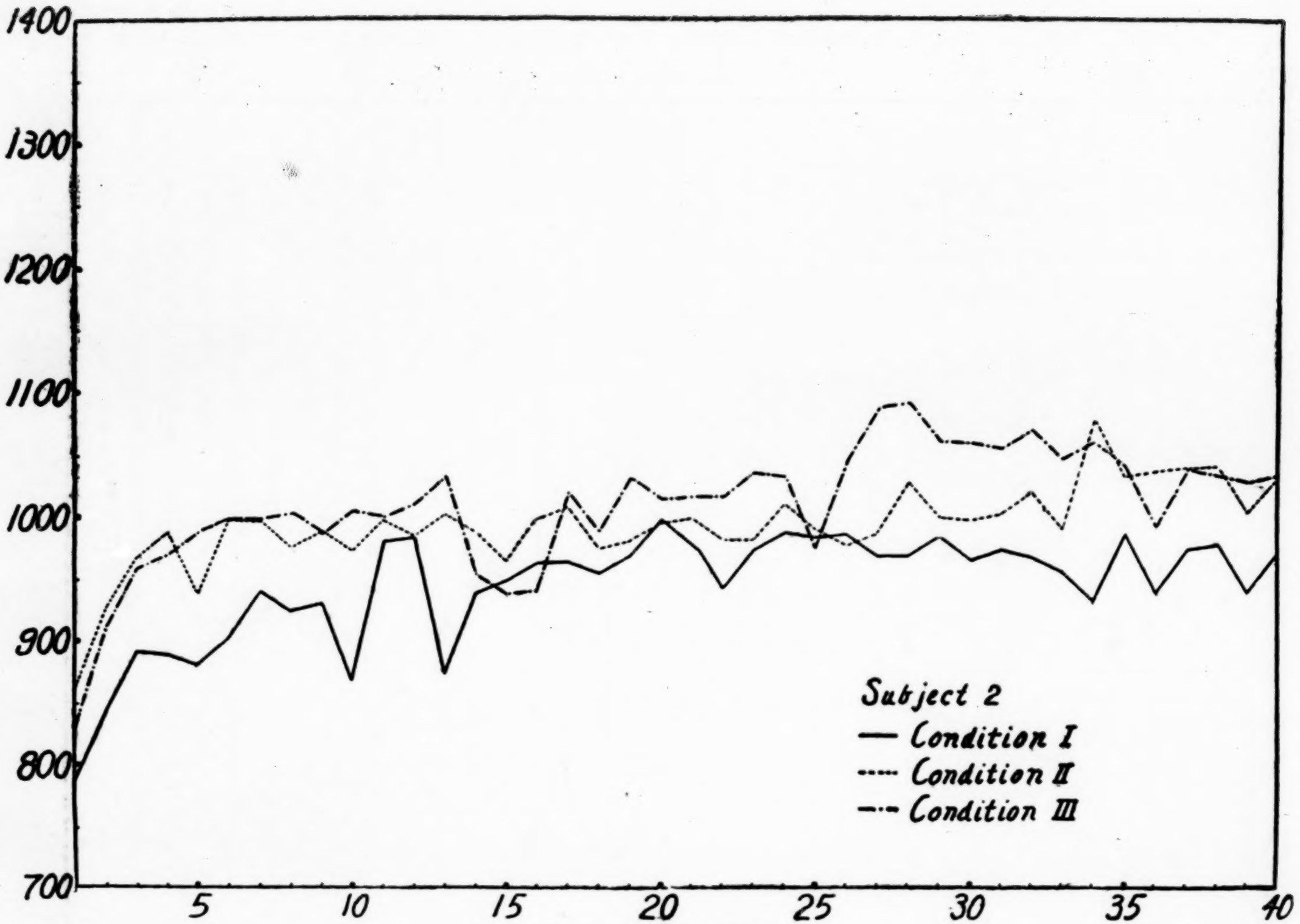

FIG. 2. (See explanation of Fig. 1.)

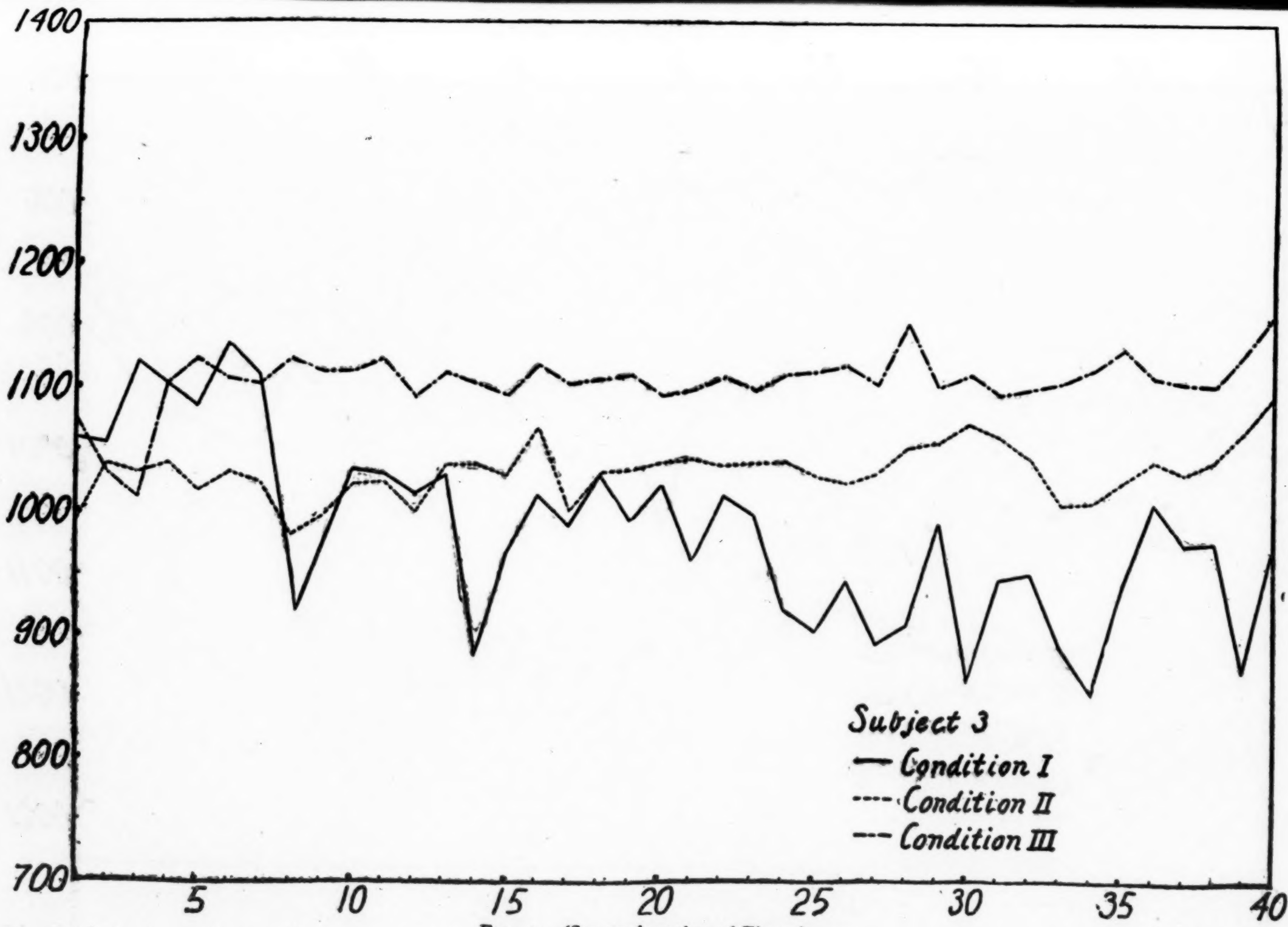

FIG. 3. (See explanation of Fig. 1.)

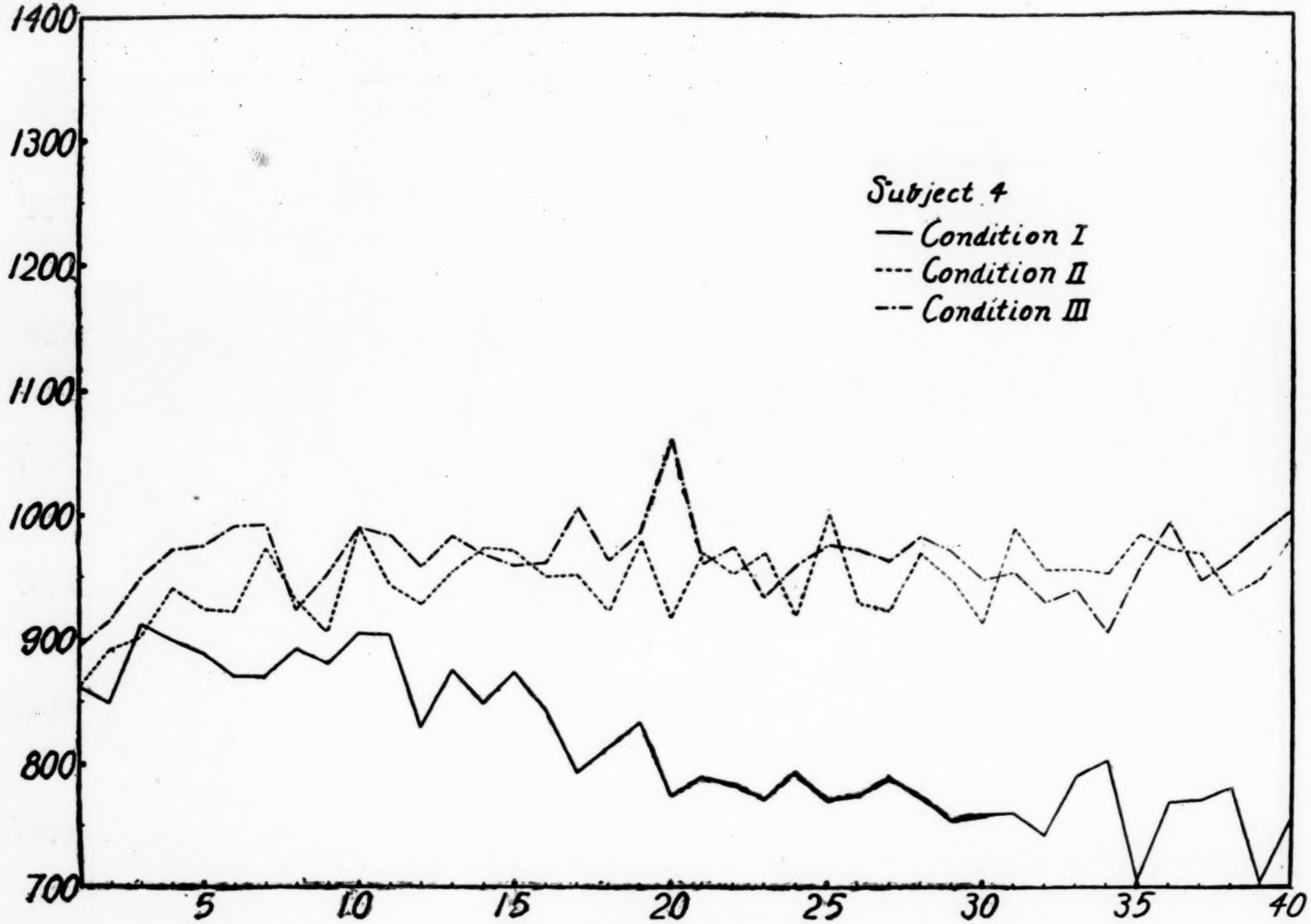

FIG. 4. (See explanation of Fig. 1.)

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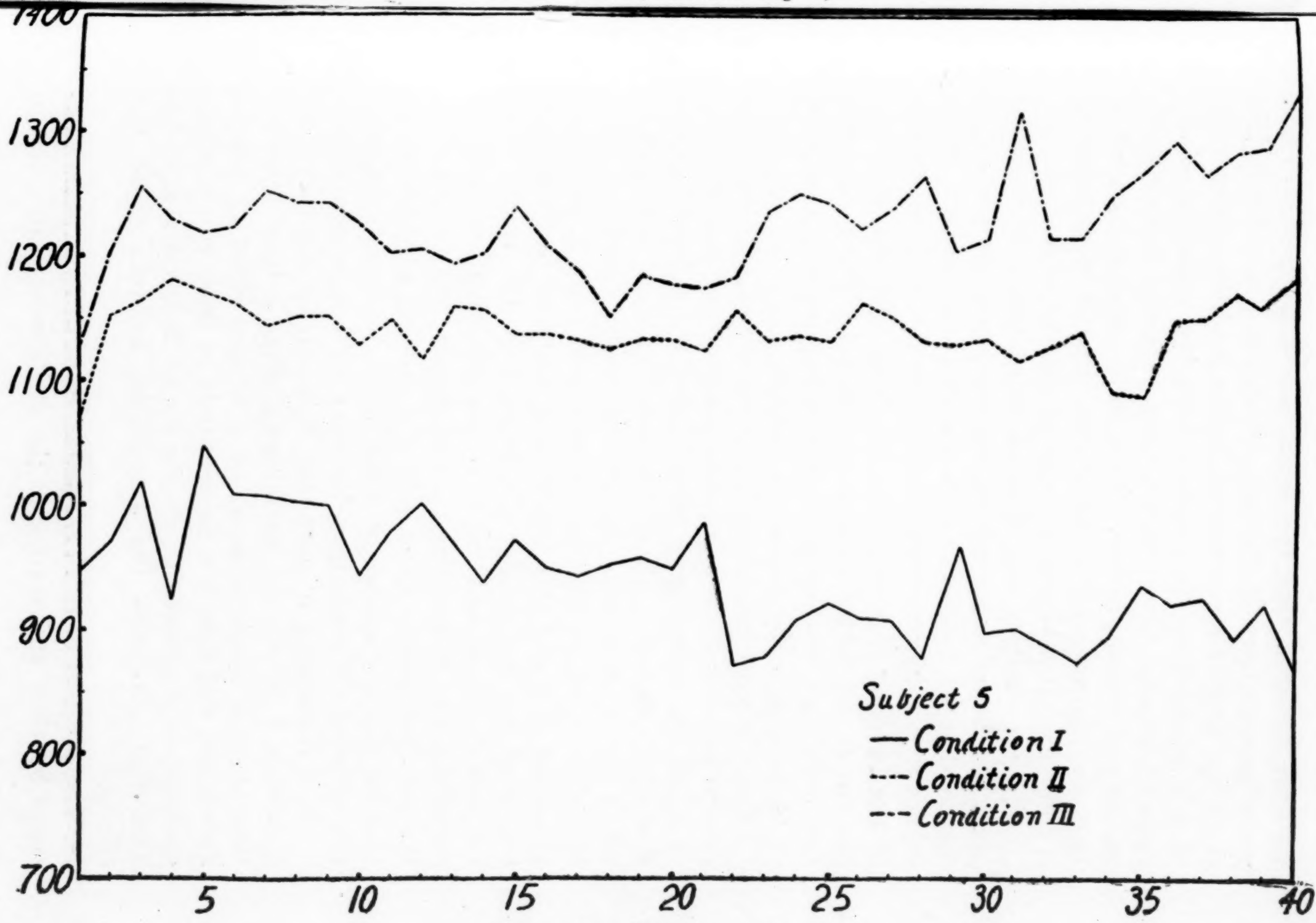

FIG. 5. (See explanation of Fig. 1.)

we have assumed that the initial rise is a genuine case of warming-up, except in the case of subject 3, Condition III., there is a greater improvement during the brief minute or so of this warming-up than occurs from the beginning of one cycle of the experiment to the next—a period containing 60 minutes of actual practice. Even in the case of that exception, the improvement in about two minutes of warming-up is equal to that achieved in about 30 minutes of practice.

We are omitting any detailed presentation of the data upon which the above statements are based for the simple reason that we believe it quite apparent on the face of the curves that subjects would not improve due to "the exercise of the function in general" at anything like the rate they improve during the brief warming-up period. This is especially true in light of the practice indulged in prior to the recorded performances of the actual experiment.

But the most important fact from the standpoint of the present paper is derived from the relative frequency of occurrence of the warming-up effect under the different experimental conditions. All of the subjects show warming-up where the work is least continuous (under Condition III.), four show it where the work is next least continuous (under Condition II.), and only two show it where the work is uninterrupted by rest (under Condition I.). Of course, more subjects would be required to render this distribution of the manifestations of warming-up statistically reliable, but since the obtained results agree with the tapping experiments in showing the appearance of warming-up to be a function of the continuity of work, they do have considerable significance.

It is interesting to note that it is only in the curves for Condition I., where the warming-up effect is least frequent, that there is much evidence of a fatigue effect or general decrement, and here it is present only in those curves where warming-up is absent, *i.e.*, the curves for Subjects 3, 4, and 5. This fact illustrates the antagonism between these two effects.

The subjects of this experiment seem to have been influenced by their anticipation of the day's work. In 4 out of

5 cases the curves for Condition I start off as low as or lower than the curves of the other conditions. This seems to indicate a disposition on the part of the subjects to save themselves when 20 minutes of continuous work was in prospect. The reports of the subjects, all of whom had considerable sophistication, show that this difference of attitude was unconscious. In this experiment, as in the one on tapping, the subjects felt their efficiency fall, or rise and fall, as the work went along, but such changes seemed entirely beyond the jurisdiction of their efforts.

Summary

The warming-up effect has been assumed to be present in non-experimental activities and in certain experimental activities where it could not be separated quantitatively from the effects of practice in the ordinary sense. Wells performed an experiment on tapping where the warming-up effect was not only present, but separable from the practice effect. Wells obtained this warming-up effect only where the tapping had a certain degree of discontinuity. Similar facts are shown in other activities by some of the Kraepelin experiments and by an experiment of Wimms. We performed two experiments on tapping under conditions quite different from those of Wells, and in both of them we secured a warming-up effect separable from the practice effect and dependent for its appearance upon the relative discontinuity of the work. We performed another experiment on the backward recitation of the alphabet. Again we obtained the warming-up effect. The frequency of appearance of the effect in this vocal experiment also varied with the discontinuity of the work.

From the experiments cited above, it seems fair to conclude that the factors which tend to produce warming-up may be present where the effect, itself, is not apparent. Probably the most frequent cause of this nullification of the warming-up effect is the presence of that complex set of factors known as fatigue, which factors are favored by the continuity of work and eliminated by its discontinuity.

PRIMACY AND RECENCY AS FACTORS IN CUL-DE-SAC ELIMINATION IN A STYLUS MAZE[1]

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The Problem

It has generally been found true that the first and last few units of serial memory material will be fixated more easily and quickly than the middle units of a series. Calkins employed series of digits, nonsense syllables, and paired colors; Finkenbinder, nonsense syllables; and Rich, nonsense syllables, poetry, and words from the Chinese vocabulary. In each of these experiments the rule was found to hold, even though in the work of Rich the subject was instructed to concentrate the attention upon the middle units of the series and disregard as much as possible the end positions. Corroborative results have also been reported by later investigators.

The present experiment is an attempt to extend the study of the primacy and recency factors to the field of trial and error motor learning, and more particularly to that type of serial motor coördination represented by the stylus maze. Upon purely *a priori* grounds it might at first be expected that these factors would operate in much the same manner in the learning of a serial complex motor pattern as in the fixation of rote memory material. However, the situation presented in the two cases is manifestly different in several important respects.

In the mastery of a stylus maze the correct response must be *discovered* and fixated unit by unit, the gradually developing integration finally becoming a single coördinated movement pattern. Memory material, on the other hand, requires merely to be fixated in the form presented, and therefore the

[1] The writer wishes to acknowledge his indebtedness to Prof. A. T. Poffenberger for his valuable suggestions and criticisms both in the designing of the mazes here used and in the treatment of the results.

response of the subject is essentially predetermined in the nature of the case, thus eliminating the necessity for trial and error effort on his part. Furthermore, the units of a series of rote memory material are presented in the simple order of temporal succession, and this order constitutes the sole pattern of the total serial response, the connections between the various units being homogeneous as well as the units themselves.

The culs-de-sac of a maze, on the other hand, usually differ from each other to a greater or less degree in type, and are unified in the total serial response by means of a spatial pattern—the true pathway, which also may vary widely throughout its course. There is, therefore, in the ordinary maze, neither homogeneity of units nor of the connections between them. Furthermore, a considerable amount of confusion as to the serial order arises in maze learning, due to the factor of retracing during the period of initial adjustment [1] in both human and animal mazes, whereas manifestly no such retracing of serial memory material is possible under the ordinary laboratory technique. Finally, the maze situation is further complicated by the relation of the spatial pattern to the particular set of arm-hand muscles used in operating the stylus, as will be brought out in the work here reported.

In the present experiment an attempt has been made to eliminate certain of these disturbing factors, by making the patterns employed as nearly uniform as could well be done and yet retain the essential motor trial and error features of the typical maze situation. In view of the extremely large differences in difficulty of various types of cul-de-sac, as pointed out by the writer in a former paper,[2] only a single type was included in the designing of the patterns here employed. Theoretically, at least, perfect homogeneity of the unit coördinations was thus secured, since the avoidance reaction pattern is precisely the same for each cul-de-sac. The connecting pathway was also made unusually simple and uniform, as the

[1] Warden, Carl John, 'The Distribution of Practice in Animal Learning,' *Comp. Psy. Monog.*, Jan., 1923., No. 3.

[2] Warden, Carl J., 'Some Factors Determining the Order of Elimination of Culs-de-sac in the Maze,' Jour. Exper. Psychol., Vol. VI., No. 3, June, 1923.

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analysis of the patterns in the next section will show. The theoretical requirement that the only variant in the pattern be the serial position of the identical units, was, however, only closely approximated.

Data bearing upon the following topics have been included in this report:

1. The primacy-recency order of cul-de-sac elimination *vs.* the regressive order.
2. The relation of the maze pattern to the right and left arm-hand muscles.
3. The factor of relative distance of culs-de-sac from the median line of the maze.
4. The relative difficulty of two maze patterns differing only in being lateral reversals of each other.

Apparatus and Method

After a considerable amount of preliminary experimentation a form of maze was constructed which was sufficiently difficult for the human subject and which involved, at the same time, only a single type of cul-de-sac. The two patterns employed are shown in Fig. I. It will be observed that pattern 2 differs from pattern 1 only in being a lateral reversal of it. The same grooved brass plate served, in fact, for both, it being necessary merely to turn the plate over from right to left, as placed for pattern 1, in order to obtain pattern 2, the quarter inch wide grooves being cut entirely through the brass plate. The quarter inch brass plate rested upon a base of plate glass which insured a smooth even surface for the movement of the stylus. The maze was approximately $10\frac{1}{4}$ by $11\frac{1}{4}$ inches across in its greatest dimensions, each cul-de-sac measuring 2 inches down the center, either right or left and one inch forward. Each segment of the true pathway opposite a cul-de-sac was exactly of the same shape and length as the cul-de-sac, the only difference being the fact that it extended in the opposite direction. The circular entrance and goal areas were each $\frac{3}{4}$ of an inch in diameter.

The maze was securely fastened to a table 24 by 36 inches in size and 30 inches high. A skeleton frame of the size of the

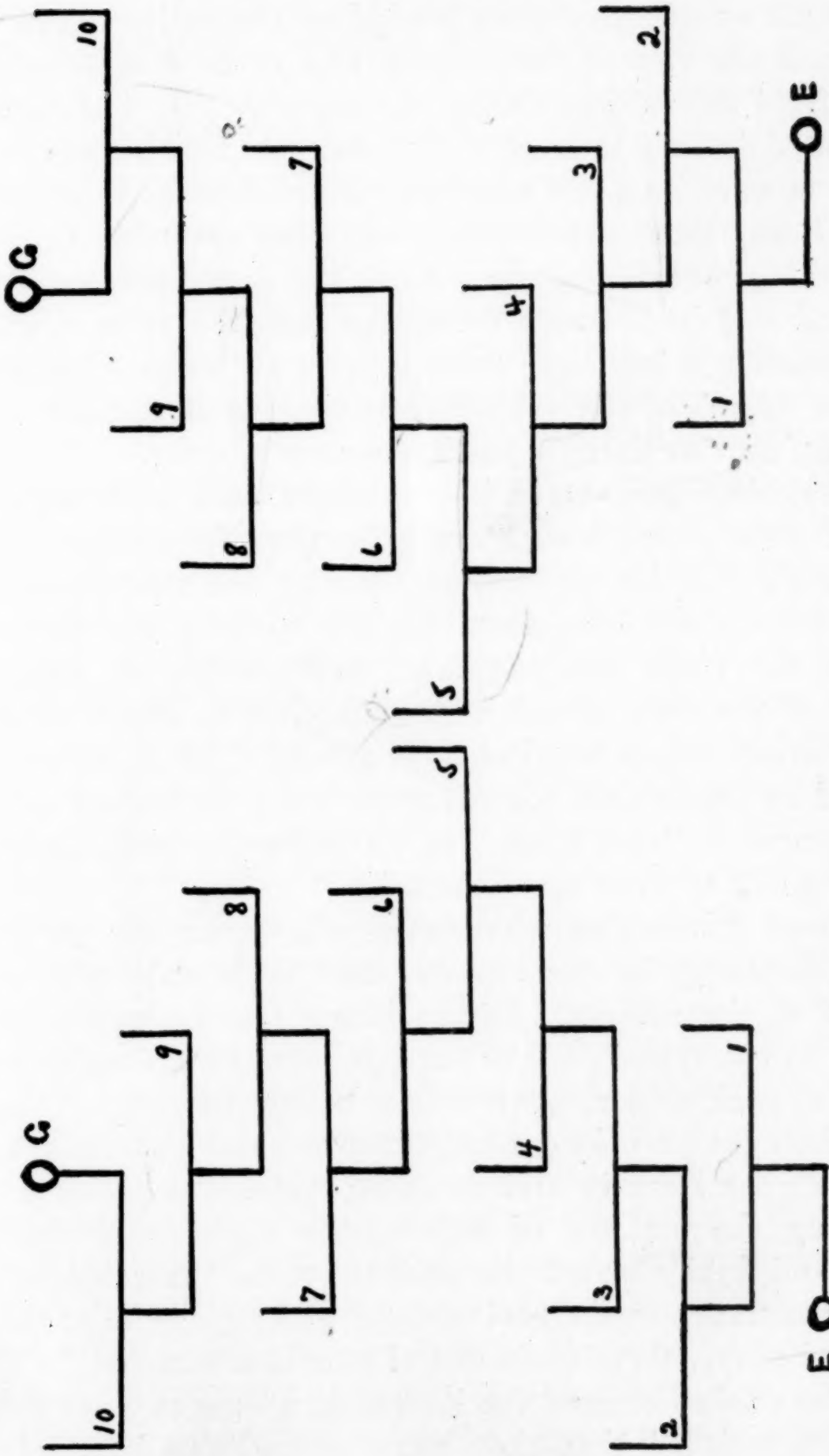

FIG. 1

table and 20 inches in height and covered with heavy black cloth made an effective hood for hiding the entire top of the table from the view of the subject, and made it unnecessary to blindfold the subject during the experiment. The curtain of the hood directly in front of the maze was held outward and slightly upward by a stiff wire in such a manner as to permit a free and easy movement of the arm, when extended through this opening, in running the maze. The gutta percha stylus used had a $\frac{3}{4}$ inch flange extending outward from a point approximately $\frac{1}{2}$ inch above the lower end, which effectively kept the fingers of the subject from coming in contact with the maze, thus avoiding a possible source of error.

The subject was seated in a straight back chair directly in front of the maze, in such a position that the midline of the maze would coincide approximately with the midline of the body. It was not considered feasible to keep the distance between the chair and the table uniform for all subjects because of the very considerable difference in length of arm that naturally occurs within a large group. This distance was adjusted by the subject himself upon being instructed by the experimenter to "seat himself in a comfortable position as if he were going to write upon the table."

No oral instruction whatsoever was given the subject, either voluntarily by the experimenter, or in answer to any question of the subject. In the latter case he was always referred to the typed page of explicit instructions which was handed to each subject upon first entering the room. These instructions gave a brief general description of a stylus maze, the signals for starting and stopping, numerous precautions concerning the manner of holding the stylus and similar details, and finally stated the problem to be "learning to go from the entrance to the goal without making any false turns into blind alleys, three trials out of four in succession." The subject was asked to read the instructions over at least twice and when satisfied that they were properly understood to indicate to the experimenter that he was ready to begin. Each subject therefore approached the problem with precisely the same opportunity for informing himself as to the nature of the task.

Inasmuch as a large body of data bearing upon individual differences in mode of attacking the maze situation was being gathered after the learning by means of a questionnaire, which findings will be reported in a later article, the rigid technique as to the pre-conditions of the learning was of special importance in this experiment.

The experimenter sat on the opposite side of the table from the subject, being practically hidden from view by the hood. Time was taken with a stopwatch. The number of entrances into each of the ten culs-de-sac during each trial was carefully checked, the projection of the stylus into a given cul-de-sac for a distance of $\frac{1}{4}$ inch or more being counted as an error. The maze was considered learned when three trials out of four were made without error. In determining the number of trials required to eliminate the individual culs-de-sac, this same criterion was used except that it was applied beyond the point at which the given cul-de-sac was last entered three times in succession. This variation in the criterion was found to be necessary because of the fact that, even after a cul-de-sac has been avoided consistently for several trials, it may again be reëntered also consistently and require to be reëliminated. The same rule was followed for all culs-de-sac, of course, throughout the entire experiment.

Only forward errors made the first time a given cul-de-sac was passed during a given trial have been included in the tables. The amount of retracing was naturally limited in a maze of such simple pattern as that employed, and the number of forward errors made on retracing excursions was so small as to be practically negligible, and these have been omitted both in the error data of the initial trial and that covering the total period of mastery. That both retracing errors and forward errors occasioned by retracing are of little or no importance in determining the relative difficulty and order of elimination of culs-de-sac in an animal maze has been previously established,[1] and the present results show that the same rule holds also for the stylus maze—at least for the patterns here studied.

[1] *Op. cit.*

Of a total of 60 subjects used in the experiment, 46 were students of Columbia College, most of whom were at the time enrolled in elementary psychology classes; 1 was a student of Barnard College; the remaining 13 were practically all graduate students of psychology in Columbia University, none of whom had previously learned a stylus maze. The subjects were divided into 4 groups of 15 each, each pattern of the maze being learned by a group using the right hand and by a different group using the left hand.

Before entering upon a discussion of the results special attention should be called to the relation between certain features of the maze employed and the nature of the problem under investigation. In the first place, inasmuch as only a single type of cul-de-sac was included, the maze presented a series of exactly similar unit spatial situations except for the fact that half were right and the other half left turns. The essential problem at each of the ten critical points was to choose between a right and left turn. Even the movement of traversing a cul-de-sac, after a wrong choice had been made, was precisely the same as the movement along the true pathway which should have been made in avoiding it, except that it extended in the opposite direction. Each cul-de-sac, moreover, was preceded by a one inch groove running in the forward direction, so that the immediately antecedent movement in approaching a cul de sac was the same throughout the maze.

In fact, the only pattern variation which the maze presented, was that occasioned by the fact that the right and left turns were not arranged in a simple alternating series. Had this been the case, then the maze obviously would have presented a series of 5 sections of 2 culs de sac each, and these sections might then have been directly compared and the effects of primacy and recency been quantitatively determined, inasmuch as both the units and the connections between them would have been altogether homogeneous for the entire series of sections. It would be easy, however, for human subjects to detect a simple scheme in which right and left turns were alternated, and it was necessary, therefore, to use a more

complicated order. The correct turn required in order to avoid the ten culs-de-sac of pattern 1 was as follows: Numbers 1, 5, 6, 8, and 9 left turns, numbers 2, 3, 4, 7, and 10 right turns. The same scheme also holds for pattern 2 except that right and left directions are, of course, reversed.

This slight variation from a scheme of simple alternation of right and left turns introduced the following irregularities into the pattern: a succession of three turns in the same direction is required in avoiding culs-de-sac 2, 3, and 4, while two such turns is required in avoiding numbers 5 and 6, and also numbers 8 and 9; Certain culs-de-sac, notably numbers 2, 5, and 10 are displaced far outward from the median line of the maze. The results indicate that this very isolation of the three culs-de-sac last mentioned served as a cue for their avoidance.

Results

1. *Primacy and Recency Factors.*—Table I. shows the initial distribution of errors for the various culs-de-sac for

Table I

Initial Distribution of Cul-de-sac Entrances under the Four Conditions of Learning[1]

Number of Cul-de-sac.....	1	2	3	4	5	6	7	8	9	10
Pattern I.—right hand.....	12	9	10	9	8	5	14	5	5	11
Pattern II.—right hand.....	10	4	7	9	5	12	5	4	12	6
Pattern I.—left hand.....	14	4	9	5	9	11	7	10	11	3
Pattern II.—left hand.....	14	12	14	9	3	6	14	4	4	11
Average—four groups.....	12.5	7.3	10.0	8.0	6.3	8.5	10.0	5.8	8.0	7.8

Average: first three, 9.9 or 66%; middle four, 8.2 or 54.7%; last three 7.2 or 48.0%

the entire fifteen subjects of each of the four groups, Table II. the mean number of errors per subject made during the entire period of learning, and Table III. the mean number of trials per subject required to eliminate each cul-de-sac. In the last line of each table the averages for the first three culs-de-sac taken as one group, the last three taken as another group, and the intermediate four taken as a third group are given.

[1] Each value in the first four lines of the table represents the total number of times a given cul-de-sac was entered by a group of fifteen subjects on the initial trial.

These averages seem clearly to support the view that, other things being equal, the culs-de-sac near either end are entered less often and eliminated much earlier than those lying in the central part of the maze. This seems to be especially true

TABLE II

MEAN NUMBER OF CUL-DE-SAC ENTRANCES PER SUBJECT FOR EACH OF THE FOUR GROUPS

Number of Cul-de-sac	1	2	3	4	5	6	7	8	9	10
Pattern I.—right hand	10.3	6.6	24.5	33.6	10.6	17.3	41.3	6.2	26.2	21.5
Pattern II.—right hand	11.1	2.5	23.1	21.6	8.7	39.4	12.6	6.5	41.8	5.7
Pattern I.—left hand	16.7	2.7	15.7	32.5	7.9	30.2	17.2	7.5	35.3	7.1
Pattern II.—left hand	13.2	8.7	19.9	34.5	8.5	18.8	38.3	5.4	28.4	11.7
Average—four groups	12.8	5.1	15.6	30.6	8.9	26.4	27.4	6.4	32.9	11.5

Average: first three, 11.2; middle four, 23.3; last three, 16.9.

of those located near the entrance, which fact suggests that primacy is of more consequence than recency in serial motor learning just as it has been found to be in fixating serial memory material. Thus, the group near the entrance was entered an average of but 11.2 times during the entire learning period; the group near the goal but 16.9 times, while the intermediate group was entered 23.3 times. The number of trials required to eliminate was 16.4 for the group near the entrance, 23.7 for the group near the goal, and 34.8 for the intermediate group.

TABLE III

MEAN NUMBER OF TRIALS PER SUBJECT REQUIRED TO ELIMINATE CULS-DE-SAC FOR EACH OF THE FOUR GROUPS

Number of Cul-de-sac	1	2	3	4	5	6	7	8	9	10
Pattern I.—right hand	11.1	9.4	31.3	44.1	30.8	30.0	59.5	6.5	51.7	34.5
Pattern II.—right hand	17.7	1.5	31.5	46.8	14.2	51.1	13.6	7.2	52.4	7.5
Pattern I.—left hand	24.1	1.5	16.0	40.0	16.2	44.9	23.7	9.9	47.1	11.1
Pattern II.—left hand	16.8	7.6	28.7	39.9	10.2	37.9	53.7	5.3	40.8	10.1
Average—four groups	17.4	5.0	26.9	42.7	17.9	41.0	37.6	7.2	48.0	15.8

Average: first three 16.4; middle four 34.8; last three 23.7.

The general tendency of the results are not disturbed by including the first four culs-de-sac in the group near the en-

trance, or by including the last four in the group near the goal, and consequently of including only three in the intermediate group—therefore the criticism cannot be made that the result arises from the particular grouping of the culs-de-sac which was used, in which four was allotted to the intermediate and only three each to the end positions.

It is also interesting to note in this connection that the end positions were not favored on the initial trial and therefore the results can hardly be due to any difference inherent in the structure of the maze itself tending to make the end positions less difficult than the middle. For, the culs-de-sac of the group near the entrance, which as shown above were mastered with less than half the effort necessary in the case of the middle group, were entered by 66 per cent. of the subjects during the first trial, whereas the middle group average was only 54.7. The group near the goal did seem to be favored by slightly fewer entrances on the initial trial as compared with the middle group. However, if the values for the two end groups be combined they show that on the average 57 per cent. of the subjects entered the culs-de-sac, on the first trial, located in the end position groups, while the corresponding value for the intermediate group is only 54.7 as above noted. The initial tendency of the subject was, therefore, to enter those culs-de-sac located near the goal and entrance, and especially the latter, more often than those lying near the center of the maze. In the final result this tendency was actually reversed as shown in the preceding paragraph, which emphasizes, of course, the importance of the primacy-recency factors.

The subjects were requested immediately after completing the experiment to fill out a questionnaire. One of the exercises required was for each to think of the maze as divided into three approximately equal segments lengthwise and to rate the entrance, middle, and goal sections as to degree of difficulty. The results are presented in Table IV. It will be seen that approximately 93 per cent. judged either the entrance or goal positions to be less difficult than the middle; over 70 per cent judged the middle to be the hardest of all,

21 per cent. gave it second place, while only 4 out of the 60 subjects considered it the least difficult. Without any suggestions from the experimenter, subjects occasionally attempted to state why the middle section seemed more difficult to them.

Table IV

Judgment of Subjects as to Relative Difficulty of Entrance, Middle and Goal Sections of the Maze [1]

Section of Maze	Entrance	Middle	Goal
Least difficult	28	4	28
Intermediate	28	13	19
Most difficult	4	43	13

Characteristic expressions were—"the pattern seems to become hazy in the middle," "the ends are easy to remember, but the pattern breaks down in the middle," "there was just one little place there in the middle that was hard for me to get," etc. The judgments of the subjects, therefore, agree with the objective data in showing that primacy and recency factors, and especially the former, are operative in the learning of a stylus maze, at least of the patterns here employed, quite as much as in the fixation of rote memory material.

II. *The Regressive Order vs. the Primacy-Recency Order of Cul-de-sac Elimination.*—It is obvious that the two maze patterns employed in the present experiment are especially adapted to test the theory, occasionally advanced, that the culs-de-sac of a maze tend to be eliminated in a regressive order. Since the culs-de-sac in both patterns are of identical type, and even the connecting pathway is extremely simple and highly uniform throughout, a more favorable set of conditions for the operation of this principle could scarcely be had.

All the evidence brought forward in the preceding paragraph in favor of the primacy-recency factors is manifestly evidence of equal weight against this notion of the regressive order of cul-de-sac elimination. For if the end positions in the series are mastered first and the middle last, as certainly proved to be the case in the present experiment, then we may properly speak of a *primacy-recency order of cul-de-sac elimina-*

[1] Based upon the ranking of 60 subjects immediately after learning the maze.

tion instead of a *regressive order.* The most unfavorable position under a regressive order *e.g.*, the position near the entrance, ranked the highest in the present instance in the final score, as previously pointed out, even though more frequently entered than any other on the initial trial.

In fact, the initial tendency seemed to be for subjects to enter culs-de-sac near the goal least of all and those near the entrance most of all, and therefore the odds at the start were favorable, not to a primacy-recency order but to a regressive order of elimination, and yet the former dominated in the final score. It cannot be contended evidently that the maze itself or the bias of the subjects operated against the principle of regressive elimination, since each of these factors are clearly favorable to the operation of this principle in terms of the objective data.

These results disagree with those of Peterson[1] who used, however, a mental maze, but are corroborative of findings previously reported by the writer[2] in an experiment with the white rat upon a maze of entirely different pattern.

III. *Relation of the Maze Pattern to the Right and Left Hand.*—So far as the writer knows, no account has been taken previously of this factor, although it would naturally be expected that, in the learning of a stylus maze in which the kinæsthetic modality is stressed, the relative difficulty of culs-de-sac would be in part determined by the peculiar manner in which the arm swings from the shoulder. This was found to be true to a marked extent in the present experiment.

After pattern 1 had been learned by a group of 15 subjects using the right hand, an examination of the data showed a consistent tendency for the culs-de-sac to the left of the true pathway to be entered more often on the initial trial than those located in the right half of the maze. This tendency of the right hand to make errors more frequently in the left half of the maze was never overcome but persisted throughout the learning period as shown by the fact that the dominant errors, or most difficult culs-de-sac, also lay upon the left side of the

[1] Peterson, Joseph, 'The Backward Elimination of Errors in Maze Learning,' Jour. Exper. Psychol., 1920, 3, 257–280.

[2] Warden, *op. cit.*

maze and were among those most frequently entered during the initial trial. The same tendency was shown in the learning of pattern 2 with the right hand although the change in pattern meant that the *right and left halves of the maze had been, of course, interchanged.*

It was decided to have each pattern of the maze learned by groups using the left hand, to see whether or not a similar tendency would manifest itself. It was found from the data obtained that the left hand tends to make errors more frequently and persistently in the right lateral half of the maze, in the case of either pattern, just as the right hand tends to make errors more frequently and persistently in the left half, and in approximately the same proportion. Table V. presents the data for each of the four groups.

It will be observed that the culs-de-sac lying on the same side of the pattern as the hand employed in learning are consistently favored, while those on the opposite side are placed at a disadvantage. That this difference is not due to the different arrangement of culs-de-sac in the two halves of the maze is shown by the fact that the rule holds even when the two halves are actually interchanged as in shifting from one to the other pattern. That the factor is relatively improtant is indicated by the fact that the differences between the 'same' and 'opposite' values of Table V. are fairly large, whether

TABLE V

EFFECT OF POSITION OF CULS-DE-SAC WITH RESPECT TO THE HAND EMPLOYED IN LEARNING

	Initial Entrances		Total Entrances		No. Tr. to Eliminate	
Relation of Series to Hand	Same	Opposite	Same	Opposite	Same	Opposite
Pattern I.—right hand	2.3	3.5	14.1	25.5	8.5	9.9
Pattern II.—right hand	2.1	2.9	13.1	21.5	6.7	9.5
Pattern I.—left hand	1.9	3.7	15.1	19.5	6.2	9.5
Pattern II.—left hand	2.1	4.0	14.9	22.6	7.4	9.3
Average—four groups	2.1	3.5	14.3	22.3	7.3	9.6

Legend: By *same* is meant those culs-de-sac lying on the same side of the true pathway as the hand being used by the subject in learning, and by *opposite* those lying on the opposite side.

comparison be made in terms of initial entrances, total entrances, or the number of trials required to eliminate the culs-de-sac of the same or opposite side.

TABLE VI

RELATION OF DOMINANT ERRORS TO THE LATERAL SECTION OF THE MAZE OPPOSITE THE HAND EMPLOYED

Initial Distribution

Number of Cul-de-sac	1	2	3	4	5	6	7	8	9	10
Position 1	.43	.35	.40	.30	.18	.18	.47	.15	.15	.37
Position 2	.40	.13	.27	.23	.23	.38	.20	.23	.38	.15

Total Entrances

Position 1	11.8	7.7	22.2	34.1	9.6	18.1	39.8	5.8	27.6	16.6
Position 2	13.9	2.6	19.4	27.1	8.3	34.8	14.9	7.0	38.6	6.4

Number of Trials Required to Eliminate

Position 1	14.0	8.5	30.0	42.0	20.5	34.0	56.6	6.1	46.3	22.3
Position 2	29.9	1.5	23.8	43.4	15.2	48.0	18.7	8.6	49.8	9.3

Legend: Position 1 is that situation in which culs-de-sac 2, 3, 4, 7 and 10 are located on the side of the maze opposite the hand employed; in position 2 culs-de-sac 1, 5, 6, 8 and 9 are similarly located.

In Table VI. the values obtained for the two hands on the two patterns are combined in order to bring together both instances in which culs-de-sac numbers 2, 3, 4, 7, and 10 are located opposite the hand employed in learning (position 1) on the one hand, and the two cases in which numbers 1, 5, 6, 8, and 9 were similarly located, on the other. The results are corroborative of those of Table V. already discussed, and therefore little further comment is needed.

Attention should be called, perhaps, to the shift in the location of the dominant errors under the two conditions. When culs-de-sac numbers 2, 3, 4, 7, and 10 are located on the opposite side of the maze from the hand used in learning (position 1) numbers 4 and 7 are the dominant errors, but when the situation is reversed and these two culs-de-sac fall on the same side as the hand being used, they assume a subordinate rôle in determining the final score, while numbers 6 and 9 lying on the opposite side (position 2) become the

dominant errors, although these latter were relatively unimportant under the former condition. In brief, the dominant errors shift from one side of the maze to the other following the rule that they always lie on the side located opposite the hand used in operating the stylus. The consistency of the individual records in this regard is indicated by the fact that when numbers 4 and 7 were in the 'opposite' position 28 out of the 30 subjects eliminated either number 4 or number 7 last; in the reverse position 29 out of 30 eliminated either number 6 or number 9 last.

It will also be noted that this factor of the relation of right or left position to the hand employed does not appear to interfere with the operation of the primacy-recency factors. The dominant errors remain in the central group of culs-de-sac merely changing from side to side without shifting to either of the end positions, so that the primacy-recency order of elimination is practically unaffected by this factor.

The tendency of the right hand to enter culs-de-sac located in the left half of the maze, and conversely for the left hand to enter those of the right half may be best accounted for, perhaps, in the natural inclination of the arm inward toward the median plane of the body as it swings normally from the shoulder. When the two hands are brought forward as in manipulating the stylus, with the elbow flexed, they normally tend to overlap, either hand extending at least a small distance beyond the median line of the body toward the opposite side. This position would operate to cause the subject, in following the grooves, to exert a greater and more constant pressure upon the left wall of the maze when the right hand was used in operating the stylus, the reverse being true when the left hand was employed.

By referring to Fig. I. it will be seen that such a tendency of the right hand to press upon and follow the left wall of the groove would inevitably cause a large increase of errors in culs-de-sac 2, 3, 4, 7 and 10 of pattern 1 and of the 5 located on the opposite side in pattern 2. The same principle would apply, of course, in the case of the left hand. That the tendency under discussion should have so strongly asserted it-

self during the initial trial would seem of special significance in supporting the explanatory hypothesis offered, inasmuch as any effect induced by the characteristic position of the hand or arm would naturally appear during the first untutored exploration of the maze.

IV. *Distance of Culs-de-sac from the Median line of the Maze.*—The fact has been previously pointed out that certain culs-de-sac, especially numbers 2, 5, and 10, were displaced far outward from the median line of the maze because a scheme of simple alternation was not followed in laying out the true pathway. An inspection of the values in the fifth line of Tables II. and III. will show that these three culs-de-sac, together with number 8, make up the four least frequently entered and most quickly eliminated. While no cause is apparent as to why number 8 should have been so consistently ignored, it is reasonable to suppose that the other three mentioned were easily avoided because of their outlying position.

The distance into each of these culs-de-sac is 2 inches to the right or left of the main body of the maze pattern—a distance great enough to be felt by the subject in terms of muscular strain in the outstretched arm manipulating the stylus. These 3 culs-de-sac increased the width of the maze from $6\frac{1}{4}$ to $10\frac{1}{4}$ inches in width, the true pathway never extending beyond the former limit. Distance cues arising from muscular strain came into play, no doubt, early in the learning, indicating to the subject when the arm had moved about far enough to include the full width of the main body of the maze only. Whatever may be the explanation, the fact is that the outlying culs-de-sac were favored as a group, and presumably because of their position with reference to the median line of the pattern.

It should also be noted in this connection that the primacy-recency order of mastery could not have been due to any unequal distribution of these relatively easy outlying culs-de sac, inasmuch as one was located near the entrance, another near the goal and the third in the central part of the maze.

V. *Relative Difficulty of Pattern 1 and Pattern 2*—A com-

parison of the data presented in Table VII. for the two patterns of maze here used will show that pattern 1 proved to be measurably harder to master than pattern 2. There is no evidence apparent in the data to indicate why this should be true. As stated previously the two patterns are exactly alike except that they are lateral reversals of each other. The difference between the values for the two patterns is fairly large; is present in the initial trial as well as in the final score, and holds consistently for either hand. The significant fact is that such a difference in difficulty does exist in spite of the similarity of the two patterns, which almost amounts to identity. Perhaps this may suggest the unreliability of making direct comparisons of the results obtained from mazes that vary greatly in general pattern, in type of cul-de-sac, and in the manner of their arrangement.

TABLE VII

RELATIVE DIFFICULTY OF PATTERN I. AND PATTERN II

	Right Hand		Left Hand		R–L Combined	
	Pattern 1	Pattern 2	Pattern 1	Pattern 2	Pattern 1	Pattern 2
Initial distribution of errors	5.9	4.9	6.1	5.5	6.0	5.2
Total errors	198.1	172.7	187.3	173.3	192.7	173.0
Total trials required to learn	73.2	67.5	70.1	64.8	71.7	66.2

Viewing the results obtained in the present experiment in the large, it seems altogether probable that the laws of primacy and recency are as applicable to trial and error learning involving a series of unit motor coördinations as to the mere fixation of rote memory material. Relative homogeneity of both units and pattern is, of course, presupposed in both cases if the typical primacy-recency order of mastery is to be obtained.

So much at least would seem to have been established, that the culs-de-sac of a stylus maze are eliminated in a primacy-recency order rather than in a regressive order, other things being equal. The former is the fundamental and characteristic order in conformity with other varieties of serial response; the stylus maze evidently does not have a law peculiar to

itself, such as is implied in the notion of the regressive order of cul-de-sac elimination. For, in the present case in which the pattern consisted of a series of highly uniform and comparable unit movements, the primacy-recency order obtained. But this is precisely the best possible set of conditions for the regressive order to have occurred. Such an order is not even suggested by the data, or any part thereof.

It is not here contended that the results of every stylus maze, regardless of its construction, should show a typical primacy-recency order of mastery. That would be as illogical as to suppose that every series of memory material regardless of the hit and miss variety of units it might contain should show such a typical order. The point maintained is that in maze learning, as perhaps in every other kind of serial response, we tend to master the first and last part before the middle, and do actually so master them provided all other conditions, except the serial position of the units, are equalized. Whether the primacy-recency factors also apply in determining the order of elimination of culs-de-sac in the animal maze can only be determined by using a design in which varieties of type of cul-de-sac are excluded and the connecting pathway made as simple as possible. So far as the writer knows, no relevant data have as yet been reported on this point.

Summary of Results

1. The factors of primacy and recency were found to operate in a typical manner in the mastery of the two patterns of stylus maze employed, the unit situations, in the case of both, being highly comparable in form and arrangement. It seems probable, therefore, that the laws of primacy and recency apply as well to trial and error motor learning, as to the mere fixation of memory material.

2. A primacy-recency order of cul-de-sac elimination rather than a regressive order obtained, the group of culs-de sac near the entrance being eliminated first, the group near the goal next, and the intermediate group last, although the maze patterns used were especially adapted for the regressive order to have occurred. The primacy-recency order of cul-de-

sac elimination would seem to be, therefore, the typical order for the stylus maze, when all conditions are equal other than the serial position of the unit cul-de-sac situations.

3. There was a marked tendency for the right hand when manipulating the stylus to enter more frequently and persistently those culs-de-sac located in the left lateral half of the maze, the converse being true also for the left hand. This was probably occasioned by the characteristic position of the arm and hand muscles in relation to the pattern, this position appearing to determine which wall of the groove should be most consistently followed, and therefore which culs-de-sac should be most frequently entered.

4. Culs-de-sac which happened to be displaced far outward to the right or left from the median line of the maze were uniformily eliminated early in the learning process, and presumably so because of their favored position.

5. Although the two maze patterns employed are identical in form, except that they are lateral reversals of each other, pattern 1 proved to be the more difficult of the two. It appears indeed significant that so small a variation in pattern of a stylus maze should measurably alter the relative difficulty of mastery.

THE RELATION BETWEEN THE TRAUBE-HERING AND ATTENTION RHYTHMS

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The possibility that the rate of the attention rhythm [1] is determined, in part, at least, by the Traube-Hering (or Sigmund Mayer) circulatory rhythm was suggested independently by Exner (2), Patrizi (4) and others. Such a relationship seemed to have been established by the work of Slaughter (6), Taylor (7), Galloway (3) and Bonser (1). Slaughter and Bonser reported a direct correspondence between the two rhythms, for some subjects. Taylor and Galloway presented data (with Sherman's results) which indicated that, on the whole, stimuli have the same effect on both rhythms. Taylor even stated that ". . . the Traube-Hering and other circulatory rhythms can be more conveniently studied in man in their secondary forms as attention waves than directly by the plethysmograph." Professor Pillsbury (5) found a diurnal rhythm in the attention wave, and Galloway reported corresponding changes in the length of the Traube-Hering wave. Altogether, the evidence seemed conclusive that the overflow effect of the vasomotor rhythm is a factor in determining the rate of the attention rhythm. Since the fatigue of the sensory or of the supporting cortical neurones may also be regarded as possible factors, it is easy to see how a periodical reinforcement from the vasomotor center might determine the rate of the rhythmical appearance and disappearance of a minimal stimulus.

It is not so easy to see how it could do so in the case of binocular rivalry or of ambiguous perspective, providing the subject maintains an impartial attitude toward the different

[1] This term is used to designate the appearance and disappearance of a minimal stimulus, the shifting from one field to another in binocular rivalry, and the changes in perspective in ambiguous perspective figures. Whether these rhythms should be called 'attention rhythms' is a question with which we are not now concerned.

interpretations. The theory would seem to require a greater reinforcing effect on the relatively less fatigued group of cortical neurones. The whole problem is also complicated by having to consider the possibility of an inhibitory effect at one phase of the circulatory rhythm, as well as a possible reinforcement at the opposite phase. There is also a difference of opinion as to whether vasoconstriction and vasodilation are both active phases, or whether one is active and the other passive. However, this impartial attitude of the subject toward the conflicting fields or interpretations in binocular rivalry and ambiguous perspectives is seldom found, and is extremely difficult to maintain. What the subject does, ordinarily, is to react to the presence or absence of one of the two possibilities.

Our interest in this problem began early in 1918, when Professor Pillsbury suggested that Slaughter's work should be repeated. This we have done, but with additional stimuli and with more subjects. The most significant of our results were obtained from the series of control experiments, which Slaughter did not have.

Method

Long kymograph records showing time, in seconds, reactions to "attention" changes, and plethysmographic and pneumographic records were taken. For the plethysmograph two long rubber bulbs were placed, one on either side of the subject's first two fingers and wrapped with a woolen band about two inches wide. These bulbs were connected by a tube with a piston recorder. The arm rested in a wide cloth sling suspended from the ceiling. This held the forearm in a horizontal position at about the level of the heart, but otherwise allowed the arm to swing freely to a comfortable position. The width of the sling was equal to the length of the forearm. The fingers hung downward over the edge of the sling. The tube from the bulbs to the piston recorder swung freely from the bulbs on the fingers, downward to a point a little above the floor, then over to the left of the subject and up to and through a hole in the wall to the recording apparatus. This arrangement made it possible for

the arm to swing without affecting the records. The subject was in one room (a dark room), the recording apparatus in another. Since Slaughter and Bonser apparently had not considered the possibility of the effect on the circulation of the reaction itself, they used either a rubber bulb to be pressed by the subject, or a telegraph key. In using either, the motor situation is quite different in the two phases of the attention rhythm, and it is possible that their results were due to this factor alone. Our reaction key was a stick to be moved horizontally, making contact when moved in one way, breaking it when moved in the other. There were no springs, nor was it necessary to exert pressure to maintain contact; so while the effects of the motor reaction were not eliminated, they were made practically the same for both phases of the attention rhythm, because there was no necessity of keeping any muscle contracted during either phase.

As "stimuli" we used visual images, minimal light, binocular rivalry, and ambiguous perspective figures. In the experiments with visual images the subject was put in the dark room and asked to image a circle, and to report each appearance and disappearance, or any regular increase and decrease in size or intensity. This task is impossible for most subjects. But with some subjects there is a regular appearance and disappearance of the image, which is projected and quite realistic. Ordinarily they are described as phosphorescent or glowing rings projected into the dark. Sometimes there is a rhythmical increase and decrease in intensity, and at other times, in size. Occasionally Go reported a rhythmical increase and decrease in the apparent distance of the ring, the motion back and forth sometimes being quite rapid. Ordinarily there was a complete disappearance at regular intervals, accompanied by some of the other changes just mentioned.

For the minimal light we used a two-volt lamp, placed in a box with an opening one half inch in diameter, covered with translucent paper. This was placed about twelve feet in front of the subject. The intensity of the light was controlled by means of resistance which was placed in the

adjoining room, making it possible for the experimenter to increase or decrease the light whenever adaptation to darkness or fatigue made a change necessary. For binocular rivalry the stereoscope was used, with colored papers about an inch square mounted upon a white card.

The ambiguous perspective figures were of the more complex sort. Individuals differ with respect to the figure which produces the best results, so to obtain the best results it was necessary to use different figures. Some figures soon became flat for some subjects. Had our main purpose been to establish the average lengths of the attention rhythm for different people and under different conditions, much valid criticism could be made of this part of the procedure. But as our main purpose was to note correspondence, separately for each attention wave, and irrespective of the length of either the Traube-Hering or attention waves, errors could not arise in that part of our work.

A series of control experiments was included, partly to test the alertness of the subjects in detecting small changes in intensity. The main purpose was to determine whether the slight correspondence found in the main series might not be due to the vasomotor reaction accompanying the motor reaction or the corresponding changes in attention. In this control series the resistance in series with the small light used for the minimal stimulus was regularly increased or decreased, thus causing the light to disappear and reappear, or the resistance set at a certain point and the circuit broken at regular intervals. In some tests the light was quite bright when it appeared, in others it was just barely perceptible. The subject reacted to these changes as to the fluctuations when the light was kept constant.

The list of subjects included Professor Pillsbury, Misses Gordon, Guernsey, Mibai, Keen, Vandertill, and Messrs. Skaggs, Seletto and Vouvakis. Only three were undergraduates. Misses Gordon, Guernsey, Keen and Vandertill, and Messrs. Seletto and Vouvakis were used during the summer of 1919. Misses Gordon, Guernsey and Mibai, Professor Pillsbury, and Mr. Skaggs during the following fall

and winter. Hereafter in this report these subjects will be designated by the first one or two letters in their names.

Results

A. Concerning a Possible Direct Relationship

No one-to-one correspondence was apparent in most of the records. Therefore, in working up the results for the series with images and minimal light, the phases of the Traube-Hering wave during which each appearance and disappearance occurred were tabulated. If there is any causal relationship between them, the greater percent of the appearances should take place during one slope of the Traube-Hering wave, and of the disappearances during the opposite slope. Or, in other words, most of the attention changes during one slope of the Traube-Hering wave should be appearances, and most of those in the other slope should be disappearances. This should be true, but to a less degree, even though the attention rhythm is much shorter than the other. Slaughter's records, which are still on record in the laboratory, were treated in the same way, and the results included in the tables for the purpose of comparison. Slaughter reported figures showing correspondences for the records taken with himself and with Professor Pillsbury as subjects, although the number of rhythms for the latter subject was but 108. With the latter records we could find no evidence of any relationship between the two waves, by either Slaughter's method of tabulation or our own, so only those of Slaughter's records where he acted as subject are included in our tables. It is possible that we did not have access to all of Slaughter's records.

We did not at first intend to use the data secured with binocular rivalry and with ambiguous perspectives in this particular connection, but since the records showed about the same relation between the two rhythms, and almost to the same degree, as the records for imagery and minimal stimulus, the results were included with the rest.

The relation between the circulatory and the attention rhythms with binocular rivalry and ambiguous perspective is

probably to be explained by a tendency for one or the other aspects to be dominant in attention. In binocular rivalry the subject may assume a more positive attitude toward the red and a more negative attitude toward the other color, and it becomes largely a matter of the appearance or disappearance of the red. This attitude is favored by the fact that the reaction stick was moved in one direction for red, in the other for green; and that for imagery and minimal light, one direction signified appearance and the other disappearance. Binocular rivalry and ambiguous perspective were not used with subjects Sk and P. With some subjects the fluctuation with binocular rivalry was so fast that it became practically impossible to trace correspondences, and in these cases the results were not used in this connection.

Deficient visual imagery made it impossible to use that with some of the subjects.

About one half of the total data was secured with visual images, and about one third of the total with the minimal light. The results are presented in Tables I., II., III., and IV. Table I. shows for each subject the number of appear-

TABLE I

Subject	No. *R*'s	Appearances in				Disappearances in			
		T	*U*	*C*	*D*	*T*	*U*	*C*	*D*
Go.....	520	112	153	114	141	81	172	132	135
Gu.....	290	50	110	52	78	55	77	68	90
K.....	175	34	56	39	46	44	36	46	49
M.....	196	38	64	34	60	26	57	39	74
P.....	261	31	107	31	92	36	86	53	86
Se.....	413	78	124	81	130	74	112	113	114
Sk.....	250	32	112	35	71	31	84	57	78
Va.....	229	30	91	36	72	29	72	37	91
Vo.....	120	11	34	19	56	24	30	23	43
S.....	261	58	99	16	88	30	60	65	106
Control Experiments									
Go.....	187	19	72	30	66	31	50	35	71
M.....	47	6	18	8	15	5	15	6	21
P.....	113	25	29	22	37	14	37	28	34
Sk.....	179	20	82	23	54	22	56	30	71

ances and of disappearances occuring during each phase [1] of the Traube-Hering wave. Read in another way, it shows the number of appearances and disappearances, respectively, which occurred in each of the phases of the Traube-Hering wave. When the data are considered in this way no fallacy will arise from the fact that ordinarily the length of the downward slope is greater than that of the upward slope. A mere glance at these figures will convince one that if the attention rhythm is affected by the Traube-Hering wave, the latter is after all a minor factor or that it is only one of many factors. Yet a close study will show that in general there were more appearances than disappearances during the first half (trough and upward slope) of the Traube-Hering wave, and relatively more disappearances in the second half (crest and downward slope). These results agree so far with those reported by Slaughter. However, it should be noticed that in general *the same is true of the control series.*

In Table II. the figures in Table I. are reduced to ratios.

TABLE II

RATIOS, FROM DATA IN TABLE I

$$\frac{\text{Number of appearances}}{\text{Number of disappearances}}$$

Subject	*T*	*U*	*C*	*D*
Go	1.38	.89	.86	1.04
Gu	.91	1.43	.76	.87
K	.77	1.56	.85	.95
M	1.46	1.12	.87	.81
P	.86	1.24	.58	1.07
Se	1.05	1.11	.72	1.14
Sk	1.03	1.33	.61	.91
Va	1.03	1.26	.97	.79
Vo	.46	1.13	.83	1.30
S	1.93	1.65	.25	.83
Control Experiments				
Go	.61	1.44	.86	.93
M	1.20	1.20	1.33	.71
P	1.78	.78	.79	1.09
Sk	.91	1.46	.77	.76

[1] In these tables *T* = trough, *U* = upward slope, *C* = crest, and *D* = downward slope.

For examples, for Go, of the attention changes during the trough of the Traube-Hering wave, 112 were appearances and 81 disappearances, a ratio of 1.38; of the attention changes during the upward slope, 153 were appearances and 172 disappearances, a ratio of .89. This treatment of the data brings out certain uniformities, particularly evident in the second and third columns, and shows that of the attention changes, for all but one subject, there were more appearances than disappearances in the upward slope, and that without exception there were more disappearances than appearances on the crest. *The same general tendency may be noted in the results of the control series.*

Table III. shows the results of adding together the changes

TABLE III

COMBINING DATA IN TABLE I

Subject	Appearances		Disappearances	
	T + U	*C + D*	*T + U*	*C + D*
Go	265	255	253	267
Gu	160	130	132	158
K	90	85	80	95
M	102	94	83	113
P	138	123	122	139
Se	202	211	181	227
Sk	144	106	115	135
Va	121	108	101	128
Vo	45	75	54	66
S	157	104	90	171
Control Experiments				
Go	91	96	81	106
M	24	23	20	27
P	54	59	51	62
Sk	102	77	78	101

occurring in the trough and upward slope, and also those occurring in the crest and downward slope. This is done separately for appearances and disappearances. Various considerations suggested this procedure, chief among which was the fact that as a general rule the pulse rate is slower at

the crest and in the downward slope than in the trough and upward slope. In many cases there is a sudden and marked decrease in rate and increase in amplitude at the beginning of the crest. The opposite of this process however is ordinarily more gradual. This method of combining the results was also suggested by a study of Table II. which shows similar tendencies for the phases grouped together.

The ratios in column "$T + U$" of Table IV. were obtained by dividing the number of appearances in the trough and upward slope by the number of disappearances. The next column ($C + D$) shows the same ratios for the appearances and disappearances in the crest and downward slope. Here a constant tendency may be seen at a glance, the only exception to the rule being in the case of Vo. Perhaps the more significant fact is that the same relation is found in the results of the control series that is found in the other.

The ratios in column App. (appearances) of Table IV.

TABLE IV

RATIOS, FROM DATA IN TABLE III

Subject	$\frac{\text{Number of appearances}}{\text{Number of disappearances}}$		$\frac{T+U}{C+D}$	
	$T + U$	$C + D$	App.	Disapp.
Go	1.05	.96	1.04	.95
Gu	1.21	.82	1.23	.84
K	1.12	.89	1.06	.84
M	1.23	.82	1.09	.73
P	1.13	.88	1.12	.88
Se	1.09	.93	.96	.82
Sk	1.25	.79	1.36	.85
Va	1.20	.84	1.12	.79
Vo	.83	1.14	.60	.82
S	1.74	.61	1.51	.53
Control Experiments				
Go	1.12	.91	.95	.76
M	1.20	.85	1.04	.74
P	1.06	.95	.91	.82
Sk	1.31	.76	1.32	.77

were obtained by dividing the number of appearances which took place during the trough and upward slope of the Traube-

Hering waves by the number of the same appearing in the crest and downward slope. Figures in the last column show the same ratios for disappearances. With but one exception, the ratios are larger for appearances than for disappearances, which means that there is a greater tendency for the appearances to occur during the trough and upward slope of the Traube-Hering waves than there is for the disappearances to occur at these times, and *vice versa*. It will be noted that this statement still holds true for the subject Se, although both ratios are less than one. In this case both appearances and disappearances occur more frequently during the crest and downward slope, but the ratios show that a greater proportion of the disappearances than of appearances are found here. Again it must be noted that the same general rule holds for the control series, the ratio for appearances being larger than that for disappearances for every subject.

In general, then, it will be seen that the figures from Slaughter's records agree with those we obtained from our own subjects. The most significant fact revealed by our own data is the fact that the same relationship was found in the data obtained in the control experiments, where the minimal light was turned on and off, the subject reacting as to the fluctuations of the minimal stimulus.

In the case of Sk there were places in the records where the correspondence between these reactions in the control experiment and the Traube-Hering wave was quite pronounced. Just recently an untrained subject was taken into a dark room and told to react one way when the small light appeared and to react the other when it disappeared. For a considerable period there was an almost exact correspondence with what appear to be Traube-Hering waves and the reactions, the light being turned on four seconds and off six. This same striking correspondence was noted one time when one of the experimenters, acting as subject in another experiment not reported here, opened and closed a knife switch every ten seconds. (For some reason a later trial failed to produce such striking results). If these were true Traube-Hering waves, this indicates a tendency for this wave to

become synchronous with the possible vasomotor effect of these periodic reactions when the two rhythms are about the same length. This might in itself explain the results obtained with fluctuating attention by Slaughter and by Bonser, as well as our own. The reason for the greater correspondence between the two rhythms, in the case of Slaughter's results, might be found in the fact that his attention wave was about the same length [1] as his Traube-Hering rhythm, and this tendency for the latter to become synchronous with the other rhythm is found only when the two are about the same length.

It is, of course, generally known that attention changes due to external stimuli or to other factors, are accompanied by vasomotor changes. It is possible that when what appear to be Traube-Hering waves become synchronous with the rhythm of the changing stimulus, we are not dealing with Traube-Hering waves at all, but with the vasomotor effects of the changing stimulus and the reaction. Yet the waves on the records, in general, seem too regular to be the results of superimposing another rhythmical effect upon the Traube-Hering, although there are places in the records which suggest that there is a tendency for a change in attention to be manifest in the activities of the vasomotor centers, and these other and separate effects would then be superimposed upon the Traube-Hering waves. This explanation would be compatible with the fact that after all there is ordinarily but slight correspondence between the two waves, it being quite impossible in most cases to detect any at all by just looking at our records; the partial correspondence showing only in the totaled results and in the ratios shown in the Tables.

It may be objected that our data were obtained from volumetric changes and not directly from blood pressure changes. In reply to such possible criticism, it might be pointed out first, that both are vasomotor and in the second place, the records show without exception that the pulse was slower in the downward slope of the vasomotor wave. This would hardly be true if some of these waves were 'active' and

[1] This might be due to chance, since, as we will see later, the rate of the attention rhythm varies with the stimulus used.

some 'passive,' with opposite relations to blood pressure changes.

B. Other Comparisons and Results

The results from comparing the records from different subjects for each of the stimuli are shown in Table V. If

TABLE V

INTER-INDIVIDUAL

Stimulus	R.		Mean		S. D.	
	Br. & Att.	T.-H. & Att.	T.-H.	Att.	T.-H.	Att.
Min. L.	.10	.10	9.8	8.8	.57	2.7
Bin. R.	.50	.00	10.9	7.8	1.37	1.9
Images	− .31	.50	9.7	11.3	.91	5.7
Ambig. P.	− .25	− .19	10.5	9.8	.82	3.32

there is any causal relationship between the average lengths of the Traube-Hering and attention rhythms, one might expect to find a correlation between the average lengths of the Traube-Hering and attention rhythms for each individual in all stimuli, and also for the Traube-Hering and attention rhythms for all individuals with each stimulus. With this idea in mind, the average length, in seconds, for the breathing, Traube-Hering and attention rhythms for each individual with each stimulus, was obtained; and upon these results Tables V. and VI. are based. The first column (Table V.) gives the method, the second, the coefficient of correlation (Foot-Rule method) between the length of the breathing rhythm and that of the attention rhythm, the third the coefficient of correlation between the lengths of the Traube-Hering waves and the attention rhythms. The next two columns give the average lengths of the Traube-Hering and of the attention rhythms for all subjects; and the last two, the standard deviations (S. D.) for the same. Since the probable error of the coefficients of correlation are large, little dependence can be placed on any one coefficient Taken together, there seems to be no evidence that the person with

the longer Traube-Hering rhythm will also have the longer attention rhythm, or *vice versa.*

It is also evident from this table that there is a greater variability between individuals in the length of the attention than there is in the length of the Traube-Hering rhythm. This holds true for each of the various stimuli used.

The data in Table VI. show that the same principles are

TABLE VI

INTRA-INDIVIDUAL

Subject	R. T.-H. & Att.	Mean		S.-D.	
		T.-H.	Att.	T.-H.	Att.
Go.....	.18	10.7	7.0	1.8	3.0
Gu.....	.18	11.3	10.1	1.9	3.0
K.....	.14	10.6	5.3	.8	1.4
M.....	.03	11.1	17.0	4.0	9.9
P.....	.10	9.1	6.4	.8	.8
Se.....	.00	10.3	8.3	.8	2.2
Sk.....	.35	10.2	6.8	1.6	.6
Va.....	.29	10.4	8.5	1.0	4.3
Vo.....	.03	9.4	6.1	1.4	2.5

found to hold for intra-individual comparisons, that is, when we compare the variations in the two rhythms for the same individual, when the various stimuli are used. In this table the second column gives the coefficient of correlation between the lengths of the Traube-Hering and attention waves. It will be noted that there is no clearly expressed tendency for the average length of the Traube-Hering wave to vary with the length of the attention wave, when the results from one subject's records for the different stimuli are compared. The other columns show the average length of all the Traube-Hering and attention waves for each subject, and the standard deviations for the same, the latter being shown in the last two columns. These figures are affected by the fact that some methods were used with some subjects more than with others. Yet most of the data obtained from the subject with the shortest average attention rhythm were obtained with fluctuating images, which Table V. shows to have the longest

attention rhythm. While there are possible sources of error to be found in the inequalities in the use of the different methods with the different subjects, two facts seem to be indicated by the data presented in Tables V. and VI. One is that there is no correlation between the lengths of the attention rhythms for either inter- or intra-individual comparisons. The second is that there is much greater variability in the length of the attention rhythm than there is in that of the Traube-Hering rhythm. Although these facts are not to be regarded as conclusive evidence against the theory that the Traube-Hering rhythm is a factor in determining the length of the attention wave, yet they do indicate that if it is a factor at all it is only one of many. It has been generally recognized that the breathing rhythm may be a factor, and if so, the list will probably have to be extended to include all the rhythms of the body.

At times the attention and breathing rhythms of two of our subjects showed an almost complete correspondence. When this occurred it was ordinarily while using binocular rivalry or ambiguous perspective, where the attention rhythm is quite short. While various explanations might be offered for this tendency, facts to be presented in the next paragraph suggest that it may be due to a tendency for two rhythms of about the same length to become synchronous.

Something of considerable interest, although it is a little difficult at present to say just what its full significance may be, was noted in the records of Vo. During the rest period which initiated any one day's work, the breathing rhythm of this subject was about three seconds long and the Traube-Hering wave about ten seconds long. But after the beginning of the first work period the two became about the same length (eight to ten seconds) and synchronous. Often the attention wave was also about the same length as the other two. The suddenness of the change in the length of the breathing rhythm was also remarkable. On one day's report the first twenty breathing rhythms were regularly about three seconds long. Then, beginning with the twenty-first, the time in seconds for the successive breathing rhythms were 5, 6, 7, 8,

8, 10, 10, 10, etc. Before the breathing reached the eight-second mark the Traube-Hering waves were regular and about ten seconds long, but from this point the two rhythms were synchronous. The length of the Traube-Hering wave was affected but little if at all, but its amplitude was greatly increased. The pulse rate became slower and the amplitude of the pulse greater at the crest, continuing through the downward slope. These changes at the crest were quite sudden, but the pulse gradually became more rapid and the amplitude less during the last part of the downward slope and through the trough, this effect continuing during the upward slope. This rhythm in pulse rate and amplitude was present before the breathing and Traube-Hering waves became synchronous, but not nearly so pronounced. The writers have no ready-made theory to explain the tendency of these two rhythms to become synchronous under the proper conditions, which seem to be the general rule for this subject while working.

At times for Vo all three rhythms, *i.e.*, breathing, Traube-Hering and the fluctuations of attention, became synchronous. At these times the appearances of the minimal stimulus or of the image usually occurred at the crest and the disappearances during the latter part of the downward slope of the same wave. This accounts for the peculiarities in the ratios for this subject in Tables II., III., and IV. It should be stated that whenever any of the other rhythms became synchronous with the Traube-Hering rhythm, it was the latter which set the pace.

To sum up this part of the discussion, it may seem that there are two ways of explaining facts brought out in our whole series of experiments. One is to regard the partial correspondence between the vasomotor and attention rhythms as the result of a tendency for the two rhythms to run together. While it may be difficult to account for any such tendency, the experiments with Vo seem to reveal its existence, at least under certain conditions. The other is to regard the circulatory rhythm, where it is clearly synchronous with the fluctuations of attention or with the changing stimulus, as

the vasomotor accompaniment of the changes which occur in the cortex. Where there is no easily detected relation between the two rhythms, but where the ratios show the operation of some constant factor, the results might be regarded as due to a super-imposing of the vasomotor effects of the attention changes upon the true Traube-Hering waves. The results of the control series obviously depend upon the attention changes which accompany the appearances and the disappearances of the light or of the motor reactions, as it is clear that here we are not dealing with any reinforcement of cortical cells from the action of the vasomotor centers of the medulla. Since the results of the control series are identical with those of the regular series with fluctuations of attention without objective change, and since in the control series the reinforcement of cortical cells by the medulla must be ruled out, why should it not be ruled out also in interpreting the results of the other series?

On the other hand the work of Sherrington and others along the same general lines would seem to indicate that the efficiency of the action of the cortex would at all times be influenced by any and all of the other nerve processes, rhythmical or otherwise. Our results do not necessarily conflict with this view. They do indicate, however, that if the Traube-Hering rhythm is a factor at all in determining even the rate of the fluctuations of attention, its rôle has been greatly over-emphasized.

It may be worth while to call attention to the fact that, as shown in Table V., the length of the attention rhythm varies for the different forms of 'stimulus' used. On the whole it is longer for the minimal light and for visual imagery and shorter for ambiguous perspective and for binocular rivalry. In no case was there any binocular color mixture unless the subject became considerably fatigued, which we tried to avoid.

Summary

1. This study is based on kymograph records showing attention, breathing, and circulatory rhythms. The attention rhythm was studied with visual images, minimal light,

binocular rivalry, and ambiguous perspective figures. The minimal light was used in the control series, in which the intensity of the light was rhythmically increased and decreased by resistance in series with it. Nine subjects were used.

2. There is some correspondence between the attention and circulatory rhythms. There is a tendency for appearances of visual images or of the minimal light to occur during the trough and upward slope of what appear to be true Traube-Hering waves, and for the disappearances to occur during the crest and downward slope. In this our results agree with those reported by Slaughter and Bonser. The control series, however, in which the intensity of the light was varied enough to make the light appear and disappear, showed the same tendencies. This means that the results obtained by Slaughter and Bonser may no longer be regarded as proving that the rate of the attention rhythm is determined by the Traube-Hering rhythm.

3. With one subject (Vo) the breathing and Traube-Hering rhythm frequently became synchronous, the Traube-Hering rhythm setting the rate at from eight to ten seconds. Under such conditions the breathing rate was only one-third of the normal rate of this subject. Twice, for periods of several minutes, the attention rhythm was also synchronous with the other two.

4. The slight correspondence between the attention and Traube-Hering Rhythms has ordinarily been explained as the result of the influence of the latter upon the former. The results of our control experiments, however, would seem to indicate that the explanation is rather to be found in the vasomotor accompaniments of the changes in attention and of the motor responses to those changes. On the other hand, the results with Vo, which reveal a tendency at times for the different rhythms to become synchronous, seem to indicate the possibility that the rate of one rhythm may be influenced or modified by another.

5. We found no correlation between the lengths of the Traube-Hering and attention rhythms. That is, there is no evidence that subjects with the shorter attention rhythm

have short Traube-Hering rhythms, or *vice versa.* Nor was there any tendency for the Traube-Hering rhythm to be shorter when using those stimuli which resulted in more frequent attention changes; nor for one to be longer when the other is longer.

6. The length of the attention rhythm varies with the stimulus used.

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RELATION BETWEEN THE QUANTITY PERCEIVED AND THE TIME OF PERCEPTION[1]

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The purpose of this study was to investigate the perception of number with particular reference to the relation between the quantity perceived and the time taken to perceive it. This problem is allied to that of the span of attention: the quantity perceived in a single, 'instantaneous' act of the mind. Sir William Hamilton has a short history of the question in his Metaphysics, Lecture XIV.[2]

William James says,[3] "The number of *things* we can attend to is altogether indefinite, depending on the power of the individual intellect, on the form of the apprehension, and on what the things are. When apprehended conceptually as a connected system, their number may be very large. But however numerous the things, they can only be known in a single pulse of consciousness for which they form one complex 'object,' so that, properly speaking, there is before the mind at no time a plurality of *ideas*, properly so called."

James McKeen Cattell[4] was the first to investigate the span of consciousness (Umfang des Bewusstseins) by careful experiment. He exposed cards on which were drawn lines 2 mm. in length for 0.01 second. "The observer had to say

[1] Research carried out at the Catholic University of America 1921–22. The writer owes much to the Knights of Columbus, whose Fellowship enabled him to study there, to the six subjects who gave so generously of their time, and to Dr. T. V. Moore, who suggested the problem and supervised the research. Much of the computation and revision of the manuscript was done by the writer while in attendance at the University of Wisconsin in 1922–23. He wishes to thank the Director of the Psychological Laboratory at that institution, Dr. C. L. Hull, for suggestions as regards computation, and for permission to use the computing-machines.

[2] Boston, 1871, p. 165, pp. 177 ff. To his references might be added, St. Thomas, 'Summa Theologica,' Part I., Q. 85, art. 4.

[3] 'Principles of Psychology,' I., p. 405.

[4] 'Philosophische Studien,' 1886, III., pp. 94–121. Wundt, 'Physiologische Psychologie,' 1902, III., p. 430. See also James, 'Principles,' I., p. 407 ftn.

how many lines he thought he had seen in each case. . . . This shows that, when 4 or 5 lines are visible for 0.01 second, their number is usually estimated correctly, which gives the number of simple impressions that can be held before conciousness at one time."

Wundt, on the basis of Cattel's and others' experiments, concludes that "A spatial idea . . . when it has only a limited extent four to six elements may be perceived at once in its totality. If it is more composite, then its parts too must pass successively through the inner fixation-point if they are to be clearly perceived. It follows therefore that composite spatial ideas (especially momentary visual impressions) are peculiarly well-suited to furnish a measure of the amount of content that can be *apperceived* in a single act, or the scope of attention. . . . Under any conditions, then, the assertion sometimes made that attention can be concentrated on only one impression, or one idea, at a time, is false. Then, too, the observations overthrow the assumption that the attention can sweep continuously and with great rapidity over a number of single ideas. The successive movement of attention over a number of objects appears accordingly to be a periodic process, made up of a number of separate acts of apperception following one another."[1]

Bertran Bourdon[2] adopted the method of reaction-times. He used dots 2 mm. in diameter arranged horizontally in various groupings, and placed 100 cm. in front of his eyes. A metal shutter in falling exposed them for a very short time, and simultaneously closed an electrical circuit through a Hipp chronoscope. Inserted in this circuit was a lip-key—*interrupteur buccal*—which was held between the observer's teeth. He used groups of dots in number from 1 to 7, with from 1 to 5 arrangements for each group, the arrangements for 3 dots being designated by 3*a*, 3*b*, 3*c*, etc. The results he obtained after 20 reactions to each dot-pattern are given for the numbers 1 to 4 in column 2 of the table on page 147. 7 was distinguished from 6 "by its crowding; there is here, then, no question of the perception of number." Additional reac-

[1] Wundt, 'Outlines of Psychology,' 1902, tr. Judd, p. 232.

[2] *RevuePhilosophique*, 65, 1908, p. 427 ff.

tions were made to the numbers from 1 to 4 only, making fifty in all to each arrangement

These figures represent the time required to perceive a number, + the time required to name it. This compound time will be represented by the symbol t_{n+p}. If we find t_n (time of naming), we can find the time of perception (t_p) by subtraction:

$$t_p = t_{n+p} - t_n$$

To find the time of naming, t_n, Bourdon replaced the dots with a 'ligne brilliante' 2 mm. x 20 mm. His assistant told him with which of the words *un*, *deux*, *trois*, *quatre* he was to react upon seeing the line. After 50 trials for each, he secured the values in sigma: *un*, 239; *deux*, 269; *trois*, 272; *quatre*, 261.

According to these figures, 20 sigma more are required to react by *four* than by *one*, and 30 more to react by *two* and *three*. Let us add 30 to the time found to react to one dot, and this becomes 334 sigma, and does not differ by more than 1/100 to 2/100 second from the time found for the reaction to two dots; in short, we finally come to the conclusion that the numbers 1, 2, 3, 4, when the conditions of perception for each one of them are favorable, are perceived in almost the same time.

Consequently experiment does not confirm the hypothesis according to which the perception of a number such as 2 is composed of two distinct acts (*deux moments distincts*): (1) The perception of the unities of the number; (2) The grouping of the unities. A number composed of certain units, such as those which were studied above, can be considered *one* sensation; 1, 2, 3, 4 are therefore sensations of the same order as blue, green, red, round, square; the quality of a group of objects is perceived essentially in the same manner as the quality red or the quality round. The operation of grouping, of the synthesis of the units of the number, considered as a psychological phenomenon, is merely a logical fiction.[1]

Bourdon and Wundt are seemingly in accord, at least as far as the perception of small numbers is concerned.[2]

Bourdon's results are open to two criticisms: (1) as a matter of fact, his figures show a small but consistent increase in the time with the number of dots; (2) the dots were arranged in regular and characteristic patterns, each pattern being reacted to fifty times, so that there was opportunity to react to a *learned* pattern rather than to number the units. Brother Antoninus attempted a more rigorous determination

[1] *Op. cit.*, p. 430.

[2] Concerned indirectly with this point is 'The Perception of Number,' by J. Franklin Messenger, Psychological Monographs, June, 1903, No. 5.

at the Catholic University Psychological Laboratory in 1917.[1] Since the present experimenter adopted Antoninus' method, the latter will not be described here. Antoninus concluded that there was an increase in the time of perception with the number of dots, the relation for 1 to 6 dots being expressed by the equation

$$\log t = kn + b.$$

His relative times of perception were as follows ($t_p(2) = 1$).

TABLE I

SHOWING THE RELATIVE TIMES OF PERCEPTION OF 1 TO 6 DOTS ($t_p(2) = 1$); FIVE SUBJECTS. ANTONINUS' RESULTS

Subject	1 Dot	2 Dots	3 Dots	4 Dots	5 Dots	6 Dots
A.....	0.755	1.0	1.47	3.34	6.12	9.53
B.....	0.72	1.0	1.43	2.68	4.33	6.98
C.....	0.758	1.0	1.49	3.01	6.03	9.41
D.....	0.825	1.0	1.23	2.51	4.53	9.09
E.....	0.627	1.0	1.51	2.83	4.48	7.65
Average.....	0.737	1.0	1.44	2.88	5.01	8.53

Clearly, if this is true, Wundt's conception of perception as a discontinuous act made up of a number of simple acts of perception, each of which is a single, unitary act into which so much can be crowded, requires modification.

II. STATEMENT OF THE PROBLEM

The disagreement between Brother Antoninus and Bourdon and Wundt suggests a re-investigation. So our chief problem is:

What is the relation between the number of objects perceived and the time taken to perceive them?

Increase in time with the number of dots might be due to eye-movement in successively fixating dots.

To what extent does eye-movement enter into the perception of a number of objects?

Then there are the queries suggested by Wundt's doctrine:

Is the perception of spatial objects of limited extent (not more than 4 to 6 simple stimuli) a single, unitary act into

[1] Submitted as a master's thesis. Manuscript in library.

which so much can be crowded, this quantity being perceived in its totality?

Is the perception of more complicated spatial objects made up of a number of successive 'simple' acts of perception?

III. Technique of Experiment and Description of Apparatus

The relation between the time of perception and the quantity perceived was investigated by the reaction-time and subtraction method:

$$t_p = t_{n+p} - t_n$$

This is not strictly true.[1]

The effects of eye-movement were studied by varying the angle which the groups of dots subtended. This was done in two ways: (*a*) by experimenting with the dots at two distances, 72 and 144 cms.; (*b*) by using scattered dot-groups and crowded dot-groups at the same distance.

Dots and figures were printed on white cards. The dots were made with ink, 1.7 to 3.2 cms. in diameter. The printed figures were about 1.8 cms. high, and centrally located on the cards. The dots were arranged haphazardly; from ten to forty cards were used for each number, so that there was no opportunity for the subjects to learn to react to *patterns* rather than to *number of dots*. The 'center of gravity' of the pattern fell at or near the center of the card.

Two kinds of dot-cards were used. On some the dots were placed inside a 2 cm. circle, on others, within a 1.2 cm. circle. These will be referred to as 'scattered' and 'crowded,' respectively.

The cards were placed in the magazine of an Ach's card-changer (Kartenwechsler), which is a device for exposing a card suddenly, and simultaneously closing an electric circuit. An electro-magnet was mounted on it, and a soft-iron armature was attached to the trigger. In this circuit was a Hipp chronoscope. This is indicated in the hook-up, Fig. 1. The current, once started, was broken by a Dunlap voice-key in series with relays. A warning signal preceded the exposure

[1] See Ladd and Woodworth, 'Physiological Psychology,' 1911, p. 407.

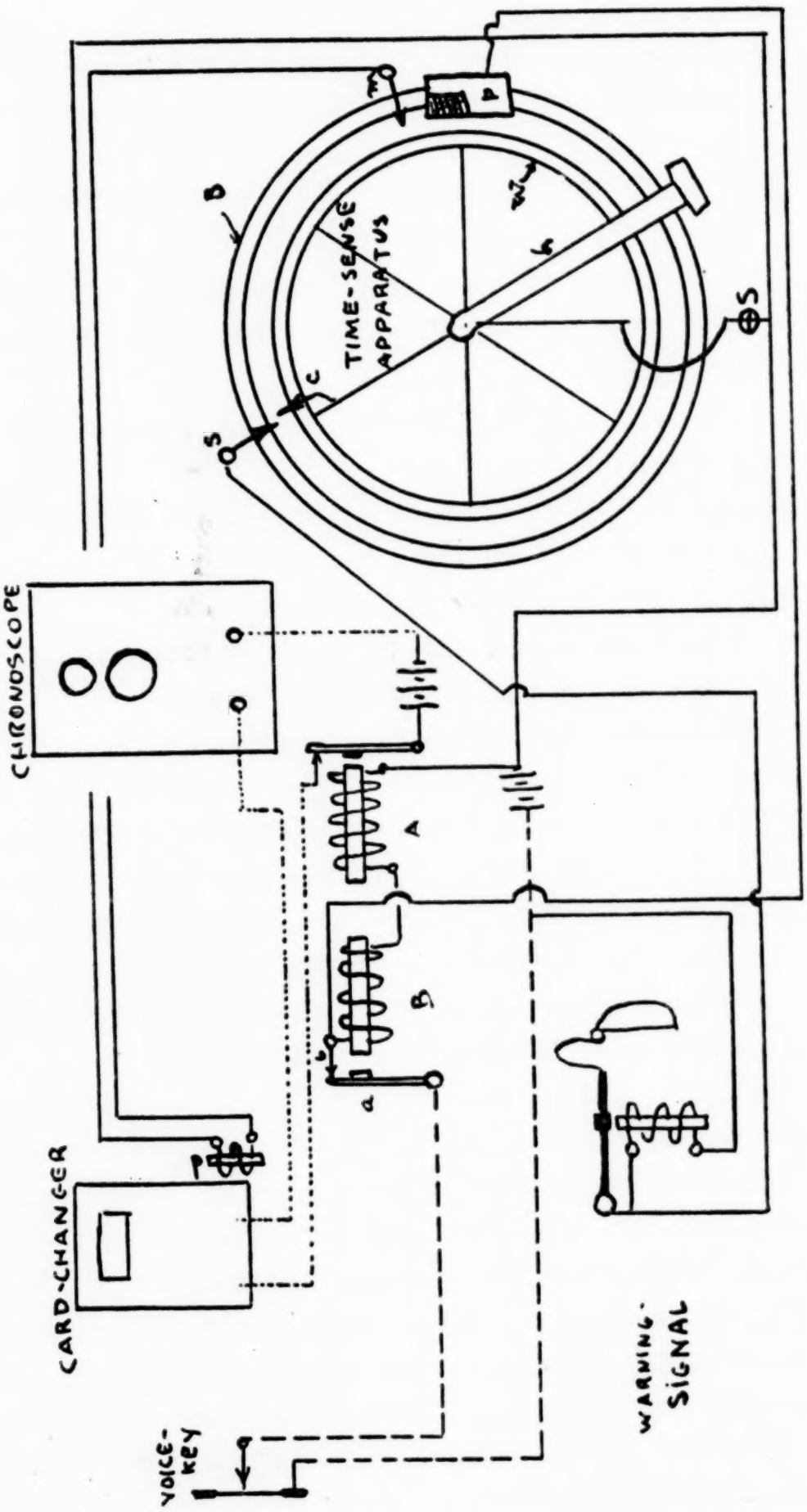
TIME-SENSE APPARATUS
CHRONOSCOPE
CARD-CHANGER
VOICE-KEY
WARNING-SIGNAL

FIG. 1

of the card by about 1.7 seconds. The card-changer, the relay *B* and the warning-bell were all operated automatically from a modified time-sense apparatus (Zeitsinnapparat) which could be wound up and released by the handle *h*. A switch *S* cuts the time-sense apparatus in and out.

Six subjects generously loaned their services. At no single session were more than 160 reactions made, in order to avoid fatigue. Though this number can be obtained in 15 minutes' steady work, some of the sessions lasted 45 or 50 minutes on account of frequent rests.

The original program involved reactions with the cards at 72 and 144 cms. A typical session's program was: (1) naming numbers printed, at 72 cms., (2) counting dots at 72 cms., (3) counting dots at 144 cms., (4) naming printed numbers at 144 cms. At subsequent sittings the order of the performances was changed at random, with the view of eliminating practice and fatigue, though, as subsequent results show, this elimination was far from complete. From 30 to 70 reactions were obtained per number per subject.

After the experiments were started, the writer adopted the rather obvious plan (but overlooked, nevertheless) of using 'scattered' and 'crowded' dot-groups, as described above. The reactions with these were taken somewhat irregularly, and so we cannot expect practice to be eliminated with these. A careful series of experiments was carried on with A.M. by this method alone.

At the shorter distance, the subjects sometimes operated the card-changer. This proved fatal, as far as the detection of eye-movement by means of a shorter reaction at the longer distance was concerned, for the activity served to keep the subjects alert at the shorter distance, whereas there was the opposite tendency, to become drowsy, at the longer distance. This objection does not, of course, apply to the detection of eye-movement by means of the 'crowded' and 'scattered' cards at the same distance.

At the conclusion of the series of sittings for each subject, introspections were taken. After each exposure was reacted to, the subject was asked to introspect, his answer being taken down in shorthand.

IV. Experimental Results

In this section the results are given. In computing averages, I used three criteria for rejecting individual reaction-times. I rejected: (*a*) all times for erroneous reactions, (*b*) all times obtained when the apparatus failed to function correctly, (*c*) times differing markedly from the "general run" of the figures, *i.e.*, by more than about 4 P.E. from the mean. In doubtful cases I sometimes gave a weight of one-half.

In Table II. are given characteristic naming-times (t_n) for one subject. The columns headed 'Reactions' give the number of reactions used in calculating the average in question, *i.e.*, after all rejections were made under (*a*), (*b*) and (*c*) above. The columns headed 'A.D.' give the average deviation. The mean and its probable error is given under 't_n.' It is apparent that, in general, t_n^{72} (time of naming at 72 cms.) is less than t_n^{144}. This is consistent with the fact that the subjects sometimes operated the card-changer at the shorter distance.

Table II

Naming-times (in Sigma)

(Subject Com.)

No.	Reactions	A.D.	t_n^{72}	t_n^{144}	A.D.	Reactions
1	21	30.1	471.0±5.5	482.2± 6.1	35.8	25
2	26	18.1	437.2±3.0	461.2± 5.4	32.9	27
3	27	32.2	461.0±5.2	470.1± 5.6	34.1	27
4	28	28.0	441.1±4.5	479.0± 7.2	43.7	26
5	25	52.1	489.6±8.8	498.0±10.0	54.3	21
6	28	37.6	492.6±6.0	518.2± 6.8	37.8	22
7	35		472	493		26
8	24		513.6	524.8		17
9	30		496	518		19
10	14		465.2	471.0		6

In Table III. are given characteristic calling-times (t_{n+p}) for one subject.

$E(s)$ represents the excess of t_p^{144} over t_p^{72} with the scattered cards; $E(c)$ represents the excess of crowded card perception-times over scattered card perception-times. t_p^c is the time of perception for crowded cards.

TABLE III

CALLING-TIMES (SIGMA)

(SUBJECT KEL.)

No.	Reactions	A.D.	$t_{n+p^{72}}$	$t_{n+p^{144}}$	A.D.	Reactions
1	42	41.2	424.0± 5.4	419.0± 2.6	23.7	61
2	50	30.7	437.5± 3.7	461.6± 3.5	28.9	50
3	58	37.6	483.7± 4.2	502.3± 4.5	39.1	54
4	59	53.9	553.1± 5.9	556.5± 6.0	58.7	68
5	49	56.0	636.2± 6.8	722.5±10.2	83.9	49
6	51	138.2	826.8±16.4	859.0±13.6	121.6	57
7	35		1189	1214		33
8	36		1060	1081		32
9	17		1451	1329		23
10	5		1124	1052		12

Characteristic perception-times are presented in Table IV. (subject Win.)

TABLE IV

PERCEPTION-TIMES (SIGMA)

(SUBJECT WIN.)

No.	t_p^{72}	$E(s)$	t_p^{144}	t_p^{c}	$E(c)$
1	65.4±18.9	−18.9±20.1	46.5± 6.2	69.6±12.3	4.2±22.6
2	60.2± 8.3	8.0± 9.3	68.2± 6.9	54.1± 8.4	− 6.1±11.7
3	142.2± 8.4	−16.1±11.9	126.1± 8.5	149.4±10.9	7.4±13.4
4	218.2±10.2	6.9±13.8	225.1± 9.2	222.2±13.6	4.0±16.7
5	337.7±13.9	73.8±24.2	411.6±20.1	335.7±16.9	− 2.1±21.1
6	513.8±15.1	42.4±21.3	556.2±15.3	479.8±23.6	− 34.9±28.1
7	630	8	638	530	−100
8	832	91	923	710	−122
9	856	133	988	1032	177
10	1153	128	1281		

In weighting the results, in order to combine them into an empirical curve, it is necessary to give the same weight to all the results for a given subject at a given distance and with given cards. I gave to each set of results a weight equal to the sum of the reciprocals of the probable errors for the numbers 1 to 6.[1] A curve for unweighted as well as weighted results

[1] I felt that to weight the results as a function of the squares of the reciprocals would give the results for some subjects an unduly low weight. Different subjects' results should be weighted as much alike as possible in order not to mask individual differences, consistently with the inherent reliability of the means. As it was, the weights ranged from .306 (For., crowded cards) to .915 (Kel., at 144 cms.).

was calculated by the method of least squares, using Brother Antoninus' relation

$$\log t = kn + b,$$

or

$$t = 10^{kn+b}.$$

The 'correlation' (used merely as a test for 'goodness of fit') between the observed values and the calculated values (weighted) is 0.9986.

TABLE V

RELATION BETWEEN THE NUMBER OF DOTS AND THE TIME OF PERCEPTION

No.	Weighted			Unweighted		
	Observed	Calculated	Difference	Observed	Calculated	Difference
1	42.2	40.6	1.6	41.7	39.6	2.1
2	70.1	71.9	− 1.8	67.5	71.0	− 3.5
3	119.1	127.5	− 8.4	119.4	127.4	− 8.0
4	231.7	226.1	5.6	237.2	228.6	8.6
5	426.6	400.8	25.8	438.6	410.3	28.3
6	688.4	710.8	−22.4	724.6	736.1	− 11.5
7		1242		1019	1321	− 302
8		2202		1100	2985	−1885
9		3905		1460	4250	−2790

$b = 1.35294$	$b = 1.34360$
$k = 0.24874$	$k = 0.25389$

As will be seen, the equation holds for 1 to 6. Beginning with 7, the calculated values greatly exceed the observed values. The equation for these six subjects is, then

$$\log t = 0.25n + 1.35.$$

A possible explanation of the increase in t_p with n is that, in the enumerating process, the eye must fixate the dots separately, moving therefore from one to the other, or moving to some extent at least.

(1) If this is the case, it would seem that the increments in t_p ought to be equal for equal increments in n, the equation being linear. But this is not so, at least for $n = 1$ to 6.

(2) It has been shown (*e.g.*, by Cattell) that it is possible to count up to 6 objects when they are exposed for too short a time to permit eye-movement.

(3) In the above tables for t_p a negative excess is in accordance with the eye-movement theory. The excess for A.M. and for all six subjects with the 'crowded' cards is given in Table VI.

TABLE VI

SHOWING EXCESS $t_p^{72} - t_p^{c}$ FOR A.M. AND FOR ALL SIX SUBJECTS

No.	A.M.	Six Subjects
1	−11.7±10.9	−13
2	− 3.2± 7.2	− 4.8
3	3.9± 9.2	0.2
4	37.1±12.7	3.2
5	−55.1±22.1	− 5
6	−60.8±22.8	−15

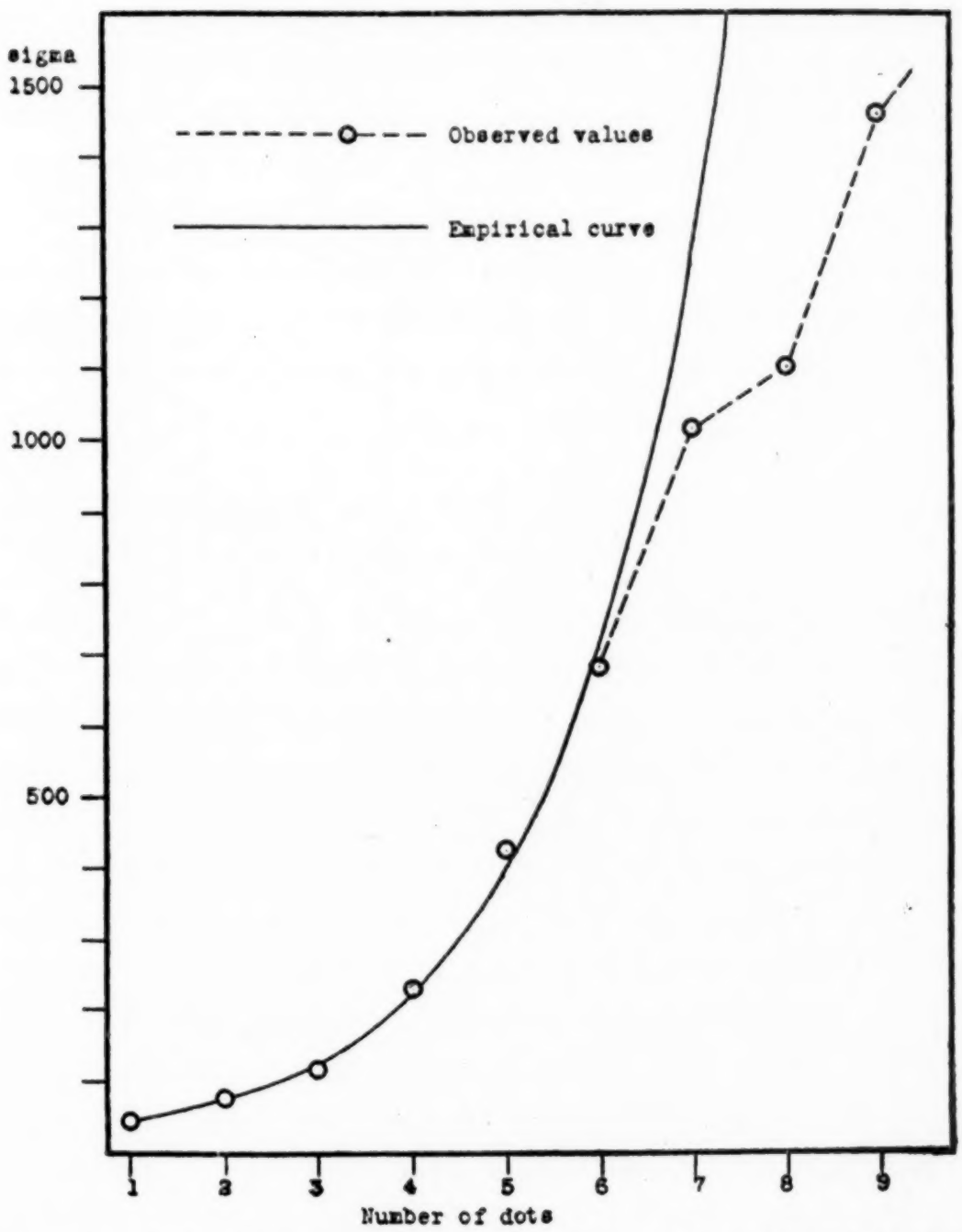

Chart showing relation between the number of dots and the time taken to perceive them (6 subjects).

The time for 1 dot is a sort of control, for theoretically it should be the same for both scattered and crowded cards. The negative excesses (− 11.7 and − 13) may mean that practice and fatigue were not perfectly balanced up, and that negative excesses are consequently to be expected, eye-movement or not. As a matter of fact, A.M. has two rather large negative excesses which are fairly reliable (reliability = 0.95 and 0.96, respectively). But their significance is materially reduced by the negative excess of 1 dot and the positive excess 37.1 of 4 dots (reliability = 0.98). Even so, the negative excess of 5 is probably too large because the reactions to the scattered cards 'got off to a bad start': the average for the first six was 1179, as against 897 for the first six reactions to the crowded cards. In a practice-series taken two months previously the reaction-times for scattered cards ranged from 589 to 787, eight reactions. The negative excesses for the six subjects are all small and of rather low reliability on account of great fluctuation in the various subjects' results. The conclusion seems to be justified that, if eye-movement is present at all for the perception of numbers up to 6, its influence is very slight if any.

V. Relation of Present Work to that of Other Investigators

Bourdon's results are presented in Table VII. The small increase of t_p with n is doubtless due to the fact that he arranged his dots in regular patterns on a horizontal line (*cp.* original article). Bourdon himself did not work out the times of perception as given in column 7, but went through the curious and unstatistical reasoning quoted on p. 4. Since Bourdon has given his A.D.'s, I have been able to treat his results as I have treated my own. I have computed the reliability of the difference between various t_p's, as given in Table VII. This shows that Bourdon's results, when properly interpreted, agree with those of Brother Antoninus and myself. His technique and interpretations are open to several other objections which will occur to the reader of the original article.

It will be noted that the empirical curve holds for 1 to 6 dots. This is probably connected in some way with the fact that the maximum scope of attention (for trained subjects) is 6. But Wundt's conclusion, that perception is a unit act which can grasp so much and no more, and which can take in 'a spatial idea . . . when it has only a limited extent . . . at once in its totality,' requires modification. Perception is clearly *continuous*. To be sure, it no longer follows our empirical law above 6, but the curve stretches on and up. Perception goes on as long as there is anything to be perceived.

TABLE VII

BOURDON'S RESULTS

No.	t_{n+p} After 20 Reactions	t_{n+p} After 50 Reactions	A. D. After 50 Reactions	t_n	t_p	Difference between $t_p(1)$ and t_p:	Reliability
1	310	304	18	239±2.6	65		
2*a*	349	341	20	269±3.0	72	(2*a*) = 7±4.73	0.841
2*b*	366	344	23		75	(2*b*) = 10±4.93	0.915
2*c*	370	354	23		85	(2*c*) = 20±4.93	0.997
3*a*	380	362	28	272±3.0	90	(3*a*) = 25±5.28	0.999
3*b*	361	366	30		94		
3*c*	383	373	34		101		
3*d*	397	405	47		133		
4*a*	403	368	40	261±2.9	107		
4*b*	365	356	24		95		
4*c*	422	393	44		132		
4*d*	417	388	42		127		
4*e*	380	367	24		106		

HIGH TEST SCORES ATTAINED BY SUBAVERAGE MINDS. III.

BY OSCAR W. RICHARDS
University of Oregon

Among the many factors that affect the accuracy of the multiple response type of test, guessing probably has the most serious effect. Tests are made wherein the maker guesses that he places the material in the right order, guesses that his rights and wrongs follow a chance order and then instructs those who take the test to guess the right answer on those items that they do not know. It never occurs to the average test user that guessing is a chance procedure which may be predicted with considerable accuracy by mathematical analysis.

Some time ago Dr. Kohs [1] published the theoretical expectancy of the distribution of scores on the two alternative test, if the subjects marked their tests solely by a chance procedure. This paper indicated that the guesser would make small scores both positive and negative rather than obtaining zero scores as had been predicted up to that time. A second paper [2] tested empirically the data of the first paper and indicated that there was good agreement between the actual scores and the theoretical prediction. This agreement becomes closer as the number of cases increases. A table was also give showing the number and frequency of runs of both right and wrong that should be included in a two alternative test of any given number of items.

This paper presents a mathematical analysis of the effects of guessing on the distribution of scales for the multiple response test and will indicate a somewhat different method of scoring this type of test.

[1] Kohs, S. C., 'High Test Scores Attained by Subaverage Minds,' *Psych. Bull.*, 17, 1920, pp. 1–5.

[2] Richards, Oscar W., and Kohs, S. C., 'High Test Scores Attained by Subaverage Minds,' Article II.

The probability of obtaining any given score on any test of the multiple response type with any given number of items, each item having a given number of alternatives, can be calculated from the binomial expansion.

$$(a+b)^n = a^n + \frac{n}{1}a^{n-1}b + \frac{n(n-1)}{1\times 2}a^{n-2}b^2 + \cdots + b^n.$$

In this case a and b represent the ratio of the number of responses. For a test with three responses to each item, a would be 1/3 and b would be 2/3. The number of items in the test would be n. Owing to the difficulty of substituting in the above expansion the following form is suggested as a general formula for use with any given test:[1]

$$p = \frac{n!}{r!(n-r)!} \times \frac{(m-1)^{n-r}}{m^n}.$$

P is the probability expressed as a decimal, n equals the number of items in the test; r equals the number of items correctly marked, and $(n\text{-}r)$ equals the number of items marked wrong. M is the number of responses to each item. Of course only one response to each item is correct, the others being wrong.

For the data of Table I., which gives the probability for a test of twenty-five items, each of which has four responses, only one of which is right, n is 25, m is 4, and p is the probability of obtaining a score of r items right and $(n\text{-}r)$ items wrong. Table II. presents the probability of obtaining any given score with a test of twenty-five items each item having three responses. The data of these tables were calculated by means of an eight place logarithm table and a table of log factorials.

From inspection of Tables I. and II. we see that the chances of the person marking the items of a multiple response test by the random procedure of guessing, will give the guesser scores most frequently of from four to ten items right. To obtain a score of all right or of all wrong is practically

[1] The writer wishes to express his appreciation to Dr. W. E. Milne of the Mathematics Department, for this particular form of the chance formula and many suggested short cuts in the calculations.

impossible by guessing. The frequency of getting other scores between these limits is indicated in the table.

TABLE I

PROBABILITY OF OBTAINING DIFFERENT SCORES BY CHANCE, USING A TEST HAVING TWENTY-FIVE ITEMS WITH FOUR POSSIBLE ANSWERS TO EACH ITEM

No. Right	No. Wrong	Score	Chance, One Chance Out of	P, Probability
25	0	100	1 125 890 182 000 000	.000 000 000 000 000 888 2
24	1	66	15 011 997 600 000	.000 000 000 000 066 773
23	2	50	416 999 950 000	.000 000 000 002 398 0
22	3	44	17 717 733 000	.000 000 000 056 440
21	4	36	1 098 841 000	.000 000 000 907 97
20	5	30	87 207 480	.000 000 011 466 9
19	6	25	10 978 751	.000 000 091 08
18	7	20	1 070 969	.000 000 933 7
17	8	17	158 658	.000 006 302 8
16	9	14	27 999	.000 357 15
15	10	12	5 833	.000 171 4
14	11	10	1 426	.000 701 3
13	12	8	407	.002 555
12	13	7	134	.007 364
11	14	5	53	.018 44
10	15	5	24	.041 67
9	16	4	14	.078 1
8	17	4	8	.124 1
7	18	3	6	.165 8
6	19	3	6	.182 8
5	20	2	6	.164 6
4	21	2	8	.117 5
3	22	1	15	.065 60
2	23	1	40	.025 084
1	24	1	160	.006 271 2
0	25	0	1 329	.000 752 55

In order to compare the relative accuracy of tests of a different number of alternatives it is necessary to use some definite criterion. One such criterion could conveniently be to compare the place in the tables that the chance of getting the score by guessing is about one chance out of twenty-five. Applying this criterion to the four alternative test we find all of the scores above twelve right on a test with three alternatives conform to our criterion. Or stating the same facts differently a person guessing would have a slight chance of marking ten items right on the four alternative type test and twelve items right on the three alternative scale. Consequently if we are to recognize guessing and its effect on the use

of our test we can be reasonably sure that only the upper fifteen items (*i.e.*, 10 to 25 right) of our twenty-five item test which has four alternatives are actually measuring the child's knowledge. He has a fair chance of guessing up to this point. Only the upper twelve items (*i.e.*, 13 to 25 right) of a test of

TABLE II

PROBABILITY OF OBTAINING DIFFERENT SCORES BY CHANCE, USING A TEST HAVING TWENTY-FIVE ITEMS WITH THREE POSSIBLE ANSWERS TO EACH ITEM

No. Right	No. Wrong	Score	Chance, One Chance Out of	*P*, Probability
25	0	100	847 288 460 000	.000 000 000 001 180 03
24	1	67	13 945 767 500	.000 000 000 059 001
23	2	50	706 073 650	.000 000 001 416 28
22	3	40	45 000 001	.000 000 022 212
21	4	33	4 186 206	.000 000 239 43
20	5	27	498 358	.000 002 006 5
19	6	24	94 109	.000 010 626
18	7	19	13 770	.000 072 619
17	8	16	3 060	.000 326 78
16	9	14	810	.001 234 5
15	10	11	253	.003 950 4
14	11	9	74	.013 564
13	12	9	40	.025 14
12	13	8	20	.050 22
11	14	7	11	.089 1
10	15	7	8	.126 4
9	16	6	6	.158 0
8	17	5	6	.167 3
7	18	5	7	.148 7
6	19	4	9	.109 6
5	20	3	15	.065 8
4	21	3	32	.031 30
3	22	2	79	.012 78
2	23	1	336	.002 970
1	24	1	2 012	.000 497 0
0	25	0	25 251	.000 039 602

the same length with three alternatives to each item are actually measuring. Applying this same criterion to a test of fifty items with two responses to each item we find that only the upper twenty (*i.e.*, 30 to 50 right) are measuring. In other words the score becomes meaningful only after the child has marked correctly thirty items, because thirty items could be marked right by guessing by about one person in thirty-six. Recapitulating if the scores are considered as measuring only after the chance of obtaining the score by

guessing is not greater than one chance out of about twenty-five we see that the three tests considered compare as follows:

Test of 25 items with 4 responses to each item only upper 15 measure.

Test of 25 items with 3 responses to each item only upper 12 measure.

Test of 50 items with 2 responses to each item only upper 20 measure.

A test of 25 items with 4 responses is about twice as accurate, as affected by guessing, as is a test of fifty items with two responses to each item. Further comparisons will be evident to the reader. (Note: the figures for the fifty-item two alternative test are taken from the paper indicated in the first footnote.)

The present method of scoring the multiple response test is to subtract 1/3 of the number wrong from the number right for the three alternative test. The main objections to this method of scoring are that it does not take into consideration the effect of guessing, it is not a scale of 100 unless the number of items is one hundred, and it does give negative scores for the lower range. The second objection may easily be met by multiplying by a constant. The other two cannot be corrected without making another scale.

The following method of scoring has been worked out to eliminate the above objections. In referring to Table I. we found that for the four alternative test the items below ten right were not measures of the person's knowledge because they might be obtained by guessing. Hence we will arbitrarily give these each one point. Referring to Table III. the first two vertical columns give the score values in number right and number wrong, the fourth column gives the assigned score value, in the fifth column this value is rounded. Only small parts of the table are shown. In the column headed first score we see that the score assigned to nine items right is nine, and for ten right is ten. The chances of one getting a score of ten right is one out of 24 but the chance of the guesser getting eleven right is only one out of 53. He has less than

half as great a chance of getting a score of 11 items right as he has of obtaining one of 10 items right. Hence instead of his being given one point more for 11 items right than for ten

Table III

Illustrating the Method of Obtaining the Score Values

Case of the Twenty-five Item Test with Four Responses to Each Item

No. Right	No. Wrong	Difference	First Score	Rounded Score	Final Score
9	16	—	9	9	4
10	15	2.2	10	10	5
11	14	2.5	12.2	12	5
12	13	3.3	14.7	15	7
13	12	3.5	18.0	18	8
14	11		21.5	22	10
24	1	75.0	146.7	147	66
25	0		221.7	222	100

right he should be given an amount proportional to the difference in the relative chances. These differences are recorded in the third column of Table III. Following this reasoning we add 2.2.to the score for ten items right and obtain the score of 12.2 or 12 when rounded for 11 items correctly marked. The differences in the chances between 11 and 12 items right is 2.5 which added to the score for 11 items right gives the value 14.7, or, when rounded, 15 for 12 items correctly marked. Similarly the difference between 24 and 25 right is 75 which added and rounded makes the score for 25 right 222. This is greater than 100 which is the most desirable scale unit so the entire scale was reduced to a scale of 100 by dividing through by 2.22. This result is the true scale values and is indicated in the last column of Table III. Scales were computed in a similar manner for the three and two alternative tests. The score values for all three different tests are given in Table IV.

TABLE IV

SHOWING COMPARATIVE SCORE VALUES FOR THREE DIFFERENT TESTS

No. Right	No. Wrong	25 Items 4 Resp. Score	25 Items 3 Resp. Score	50 Items, 2 Responses			
				No. Right	Score	No. Right	Score
25	0	100	100	50	100	25	14
24	1	66	67	49	73	24	13
23	2	50	50	48	59	23	13
22	3	44	40	47	54	22	12
21	4	36	33	46	47	21	11
20	5	30	27	45	42	20	11
19	6	25	24	44	38	19	10
18	7	20	19	43	34	18	10
17	8	17	16	42	32	17	9
16	9	14	14	41	30	16	9
15	10	12	11	40	27	15	8
14	11	10	9	39	26	14	8
13	12	8	9	38	24	13	7
12	13	7	8	37	22	12	7
11	14	5	7	36	21	11	6
10	15	5	7	35	20	10	5
9	16	4	6	34	19	9	5
8	17	4	5	33	18	8	4
7	18	3	5	32	18	7	4
6	19	3	4	31	17	6	3
5	20	2	3	30	16	5	3
4	21	2	3	29	16	4	2
3	22	1	2	28	15	3	2
2	23	1	1	27	15	2	1
1	24	1	1	26	14	1	1
0	25	0	0	0	0	0	0

These scores are based on a scale of one hundred units so arranged that all wrong gives a zero score and all right a score of 100. The values for various numbers right are proportionate to their measuring value according to a strict interpretation of probability. However from the practical standpoint it is not desirable that the difference for the four alternative test between 24 and 25 right be as great as 44 units, even when this is correct if we consider only the factor of their measuring value as affected by guessing. In order to make a scale that is more practically accurate we can average this scale with some other scale. If we use the formula $4\ (r - 1/3w)$ for our scale we find that the values near fifteen right are over valuated. That is, the difference between the two values is greater

than it should be as these scores could be obtained by guessing, while the difference between the higher values, *e.g.*, 24 to 25 is under valuated because it has the same unit as from 15 to 16. Hence if we combine such a scale with our scale constructed from the probability standpoint only, we will get a more practically accurate scale. It might even be desirable to weight the ordinary scale and then average the two. One investigator [1] suggests weighting the right minus wrong scoring method by multiplying by five and then averaging the result. This gives a desirable scale from the practical standpoint that does consider the possibility of obtaining scores by guessing because the upper score differences are greater than those in the center of the scale and yet does not over emphasize guessing by taking it alone into consideration. Table V.

TABLE V

WEIGHTED SCALE FOR EDUCATIONAL USE

No. Right	No. Wrong	Prob. Score	$4(r - 1/3w)$ Score	Educ. Score
25	0	100	100	100
24	1	66	95	89
23	2	50	89	81
22	3	44	84	76
21	4	36	80	71
20	5	30	74	65
19	6	25	68	60
18	7	20	64	55
17	8	17	58	50
16	9	14	52	45
15	10	12	47	40
14	11	10	42	35
13	12	8	36	30
12	13	7	30	25
11	14	5	25	21
10	15	5	20	16
9	16	4	14	12
8	17	4	9	8
7	18	3	4	4
6	19	3	−1	3^2
5	20	2	−6	2^2
4	21	2	−12	2^2
3	22	1	−14	1^2
2	23	1	−21	1^2
1	24	1	−28	1^2
0	25	0	−34	0^2

[1] Dr. C. A. Gregory, Department of Education, University of Oregon.

[2] The probability score value is used here to obviate negative scores.

gives a scale obtained in this manner. The probability is only given 1/5 the total value. Other scales can thus be readily made by the test maker evaluating the guessing factor as he thinks it should be weighted by combining the scales computed purely on a guessing basis with any other scale he desires to use.

Summary

This paper shows how chance affects the use and scoring of the multiple response test. Tables are given showing the probability of a guesser obtaining any given score with a multiple response test of twenty-five items with either three or four alternatives. The relative accuracy, as affected by guessing, of the two, three and four alternative tests are compared. A scoring method and scales are presented taking into consideration the relative measuring value of the items according to how easily they could be marked right by the guessing procedure. A method is suggested whereby the guessing factor can be combined with other factors in preparing a more satisfactory and practical scoring method for this type of test.

RECORDING APPARATUS: THE ELECTRO-KYMOGRAPH

BY ADELBERT FORD

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Confronted with the problem of taking pulse and vasomotor graphic records for half-hour intervals, we found the usual smoked paper kymograph methods inadequate. Of course it has been possible to take records over a considerable period of time by gluing the paper into an endless belt, mounting it on two upright drums, and moving the recording needles in such a manner as to produce a spiral record several times around the entire circumference of the belt. This operates especially well when there are but two or three needles, but when five or more recording needles are being operated simultaneously it is possible to make, at most, but two complete revolutions of the paper belt. It is especially difficult to use this spiral form operating cardiographic and pneumographic recorders because of the wide path these require for their maximum movement. A plethysmographic needle, running simultaneously with a pneumograph, and four reaction pointers require a paper at least 15 cm. wide, or twice as wide if it is desired to make a spiral record of two complete turns. When the paper is running very slowly the recording line cannot exhibit all the slight and rapid variations of the needles; when the paper is running rapidly it is impossible to get a continuous record for a half hour experiment.

Other objections than the time obstacle are inevitable with the smoked paper methods. Sooting the paper with oil or gas lamps, and the fixing of the smoked surface in shellac are unhealthful to the experimenter and usually require more time than the actual taking of the records. Long endless belts of smoked paper are exceedingly difficult to manipulate without injury, especially in the process of dipping in the

shellac. Outside of the mere mechanics of manipulation, other factors are of grave concern to the scientific interests in precision. We found that the very light needle of the Lombard-Pillsbury plethysmograph could not be adjusted to the smoked surface with a constant pressure. Variations in the friction of the needle point against the paper are certain to produce variations in the lateral swing of the pointer as a whole. A sudden shallowness, for instance, in the depth of the pulse notches may be attributed wrongly to organic changes in the physiological functioning of the subject when, as a matter of fact, the shallowness is merely the failure of the needle to register freely due to an increase in friction. Such increases are always certain to occur when the paper is ever so slightly wrinkled and so does not lie flatly on the drum.

It is perfectly possible to produce graphic records by casting the shadows of the moving pointers on sensitized paper,—a method which would eliminate, most certainly, all friction,

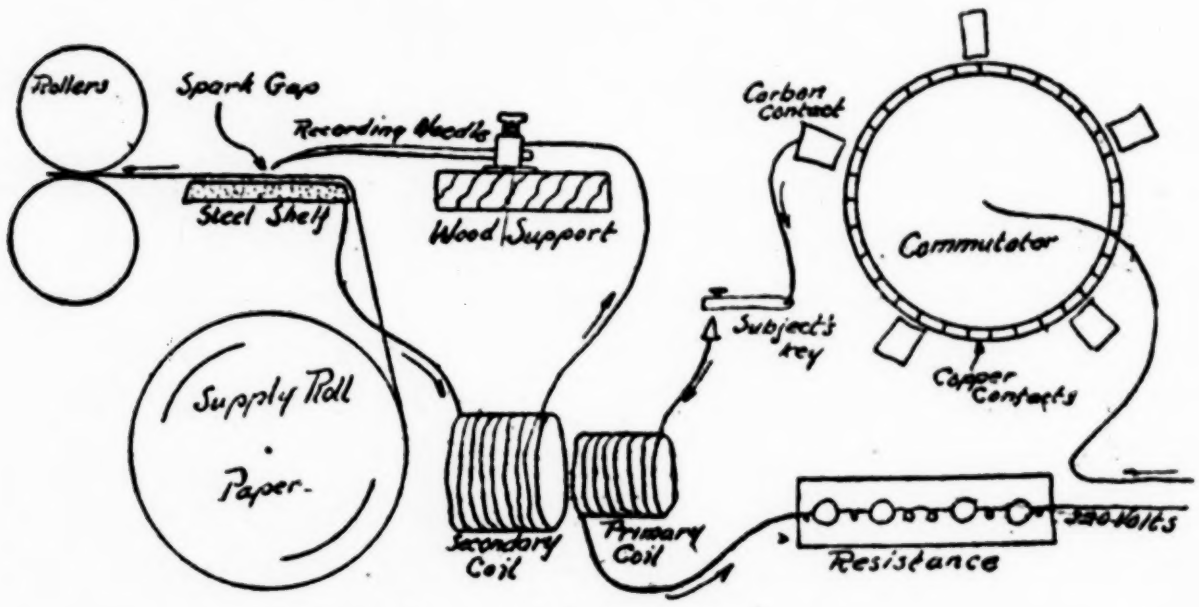


Diagram of electro-kymograph.

and at the same time enable the experimenter to continue his experiment over any period of time by drawing the recording paper from a large roll instead of using the endless belt method. It will be obvious that, with records 15 cm. wide, the cost of furnishing paper for several hundred records would be prohibitive for the average laboratory.

Ink needle methods have been tried successfully with less delicate types of pointers, but the mechanics of the ink

recorders are inapplicable to such delicate instruments as the Lombard-Pillsbury plethysmograph. A drop of ink is several times heavier than the entire needle of this instrument and would present an amount of inertia too great to be overcome by the pressure in the pneumatic tube. The ink method, therefore, can be used in combination with some of the other methods which we are about to illustrate. We have secured good results by attaching an ordinary fine-point steel Gillott or Spencerian point on an aluminum indicator and dipping this in a free flowing fountain-pen ink. Due to the very fine line, the flow of ink from a drop on the pen point will generally last for more than a half hour. If it does not the pen can be re-loaded during the actual course of the experiment by the use of a glass dropper. Such pen points can be used on reaction needles and on Marey Tambours, if the movement of the points is not too violent. The points should be detachable and wiped before each experiment.

In order to solve the problem of eliminating the friction from the point of the Lombard-Pillsbury plethysmograph, we turned to an electrical means of writing the graphic line, that is, by the discharge of a series of high frequency sparks directly through the kymograph paper. This produced a series of small holes punctured by the sparks and so constituted a graphic line which looked as if it were composed of fine dots about .5 mm. apart.

The electrical apparatus consisted of the following parts:

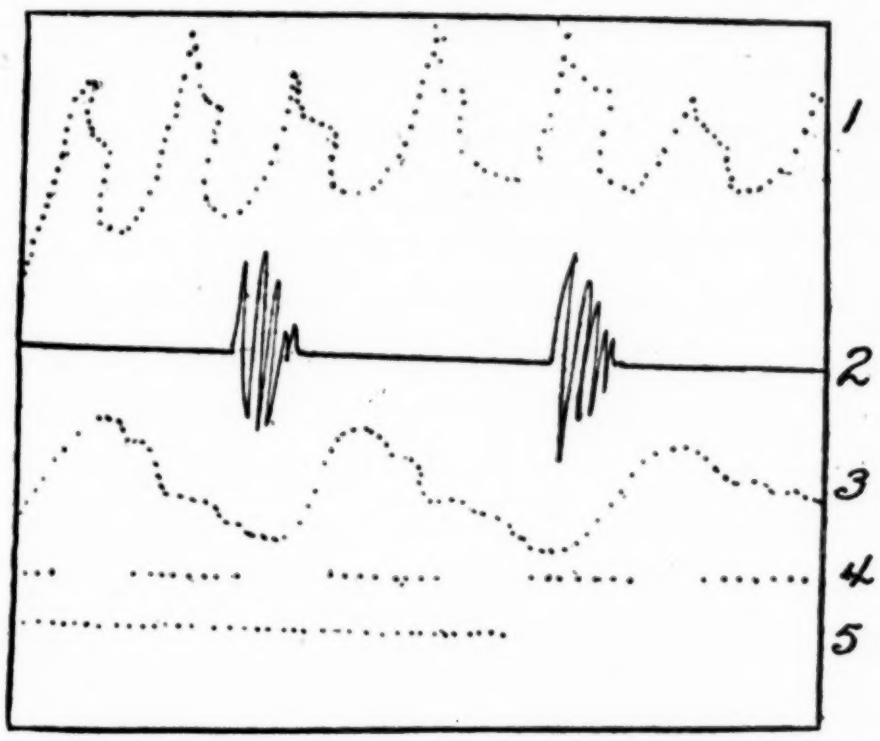
The Kymograph.—Since we could now use white, unsmoked paper, we built a kymograph so constructed that the paper was drawn from a large roll mounted on a spindle. The paper passed from this roll over a horizontal steel shelf and under the recording needles, being pulled between two power-driven, rubber-coated rollers. The recording needles were mounted above the metal shelf so as to swing horizontally, not vertically, as has been very frequent in laboratory practice. With the usual vertical mounting of the piston plethysmograph needle it is necessary to counterbalance the back of the pointer so as to overcome the gravity pull on the point of the needle. By the use of the horizontal position

we were able to use aluminum pointers which weighed as low as .02 gm. For reaction needles we used sharpened wire pointers fastened rigidly to the apparatus support. It is not necessary to have movement of these needles, since the reaction can be indicated by starting and stopping the passage of sparks by means of a switch in the hand of the subject. One tambour was operated by the ink-needle method and another by the spark method. Altogether we had one plethysmographic needle, two tambour needles, and three reaction pointers operating simultaneously. Because the sparks were produced by high tension electric discharge, the various parts of the kymograph were carefully constructed with a view to proper insulation. The instrument board was made of seasoned maple on which were screwed steel clamps for the support of the reaction devices. A space of 2 cm. was left between each of the clamps to prevent cross discharge of sparks between each pair of instruments. The entire kymograph was now enclosed in a wood and glass case as a protection to the experimenter.

The Transformers.—Step-up transformers were made from ordinary gas-engine coils. 220 volts of direct current from our power house, adjustable from .5 to 1.5 amperes by the use of a lamp-bank resistance, was conducted to the five primary coils. The regular vibrator devices on the ends of the coils were taken off and a rapidly revolving, copper-point commutator was substituted as a means of breaking up the current into small impulses. The commutator was equipped with forty copper points and carbon brush contacts to insure reliable performance. The copper points were so wired that the impulses were equally and alternately distributed to the five coils, that is, each coil received every fifth impulse with its full value in amperage. Since the commutator, attached to a quarter horse-power motor, revolved thirty times per second, the secondary coils, therefore, produced 480 sparks per second from the needle-points. The time reaction was indicated by placing a switch, operated by the subject, in the circuit of one of the primary coils to start or stop the passage of sparks. By inserting a mercury seconds clock

in the circuit of one of the primary coils a periodic discharge of sparks from the wire pointer served as a means of measuring time in seconds. Although we used 220 volts of direct current because that was already wired to our rooms, we found by further experimentation that the coils could be operated on as low as 32 volts of battery current. Lower voltage proved to be inadequate because of the uncertain passage through the carbon contacts.

The records produced on this apparatus proved to have eliminated several of the objectionable features of the smoked paper devices. It was especially advantageous in the case of the piston plethysmograph that the needle move without friction, about .5 mm. above the paper. There was a slight attraction between the needle point and the steel shelf, due



1, 3, 4, 5 are taken with the spark method, 2 with an ink needle.

to opposite static charges, but as this force was exerted in a direction perpendicular to the path of the needle movement it could not appreciably hinder freedom of movement. The pulse notches in these blood circulation records showed an amplitude about three times that of the average smoked paper record. When a study of pulse form was desired, the speed of the paper was increased to any desired extent in order to stretch out each pulse notch, and this could be done

without any limitation in the length of time covered by the record since the paper was drawn directly from the supply roll. All the time involved in smoking and fixation by the old smoked paper process was thus saved for concentrated experimental work.

It has been objected that this type of graphic record does not show the fine and precise course that can be obtained by the smoked paper process. But the increased amplitude of the needles, and the regularity of the movement, free from friction variables, more than compensates for this difference. Diacrotic notches in the pulse form, often very difficult to obtain by the use of the smoked paper method, are easily secured from the majority of subjects on this apparatus. The noise factor which was suggested as an objection comes from three sources: the action of the commutator, the vibration of the motor, and the crackling of sparks as they pass through the paper. This objection was practically eliminated by placing the motor and commutator in a sound proof box, and the kymograph inside a glass and wood case. With these precautions the distracting sounds could not reach the subject through an ordinary laboratory partition. It is possible, also, to place the motor and commutator in another room and wire, through conduits, the connections to the coils, which should be as near the recorders as possible. The kymograph, in this case, may be operated by the use of the spring motor, or the practically noiseless Dunlap synchronous motor.

In general the apparatus presented certain advantages which might appeal to the experimenter accustomed to the more complex recording devices. Since the recording needles did not actually touch the kymograph paper, friction adjustments were entirely eliminated. The reduction in the number of apparatus adjustments in the course of the experiment enables the operator to spend more time in actual observation of other aspects of the experiment. We found it highly satisfying to have this apparatus always ready whenever sudden opportunities for tests arose; the only preparation

necessary consisted in filling the ink needle, oiling the piston recorder, and turning on two electric switches for the motor and coils. The saving in time by the elimination of smoking and fixing processes enabled us to go through a much larger number of experiments and get a much larger group of cases on which to compute results, a factor which is being insisted on more and more as statistical mathematics is playing such an important rôle in psychological technique.